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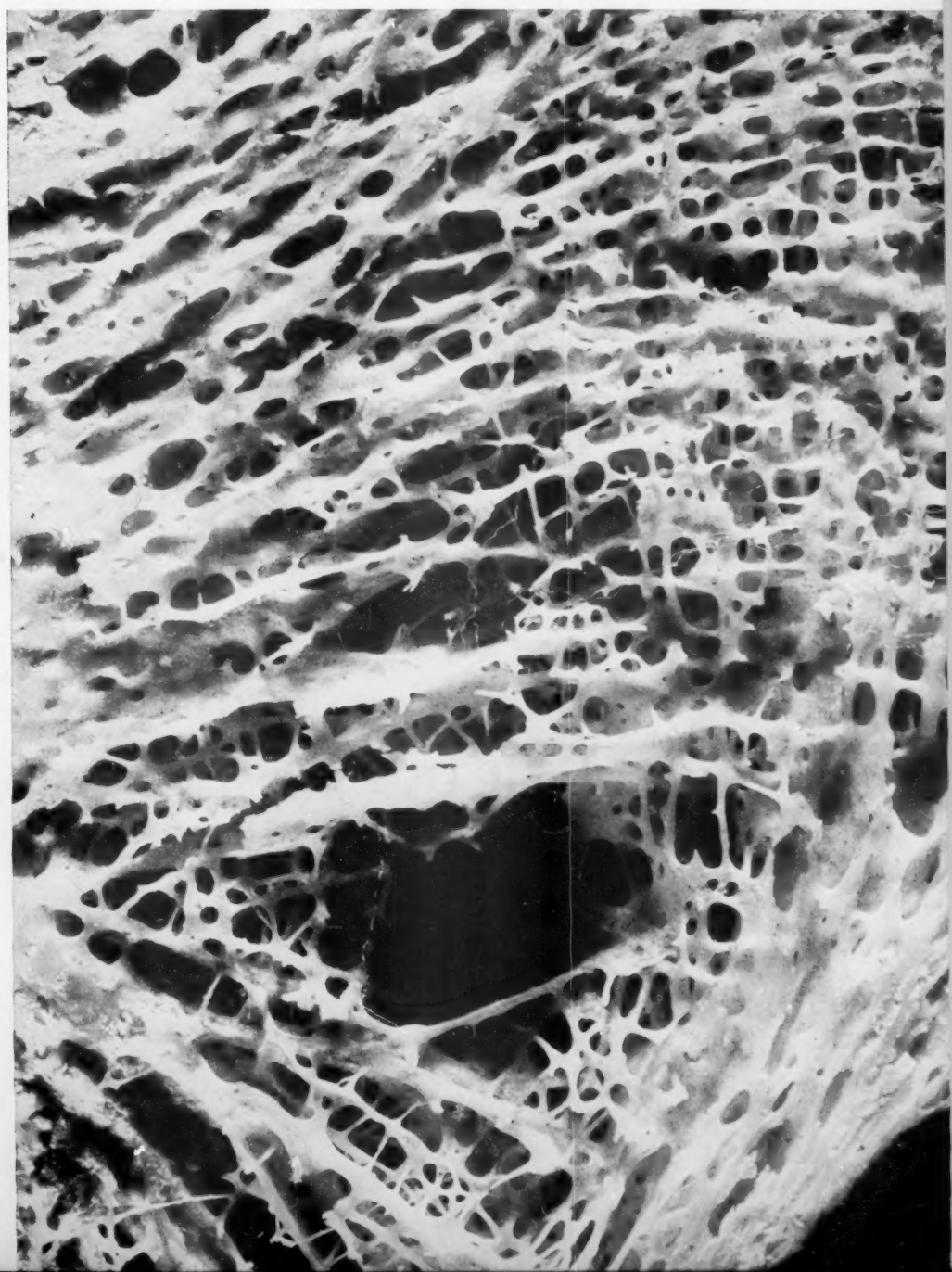

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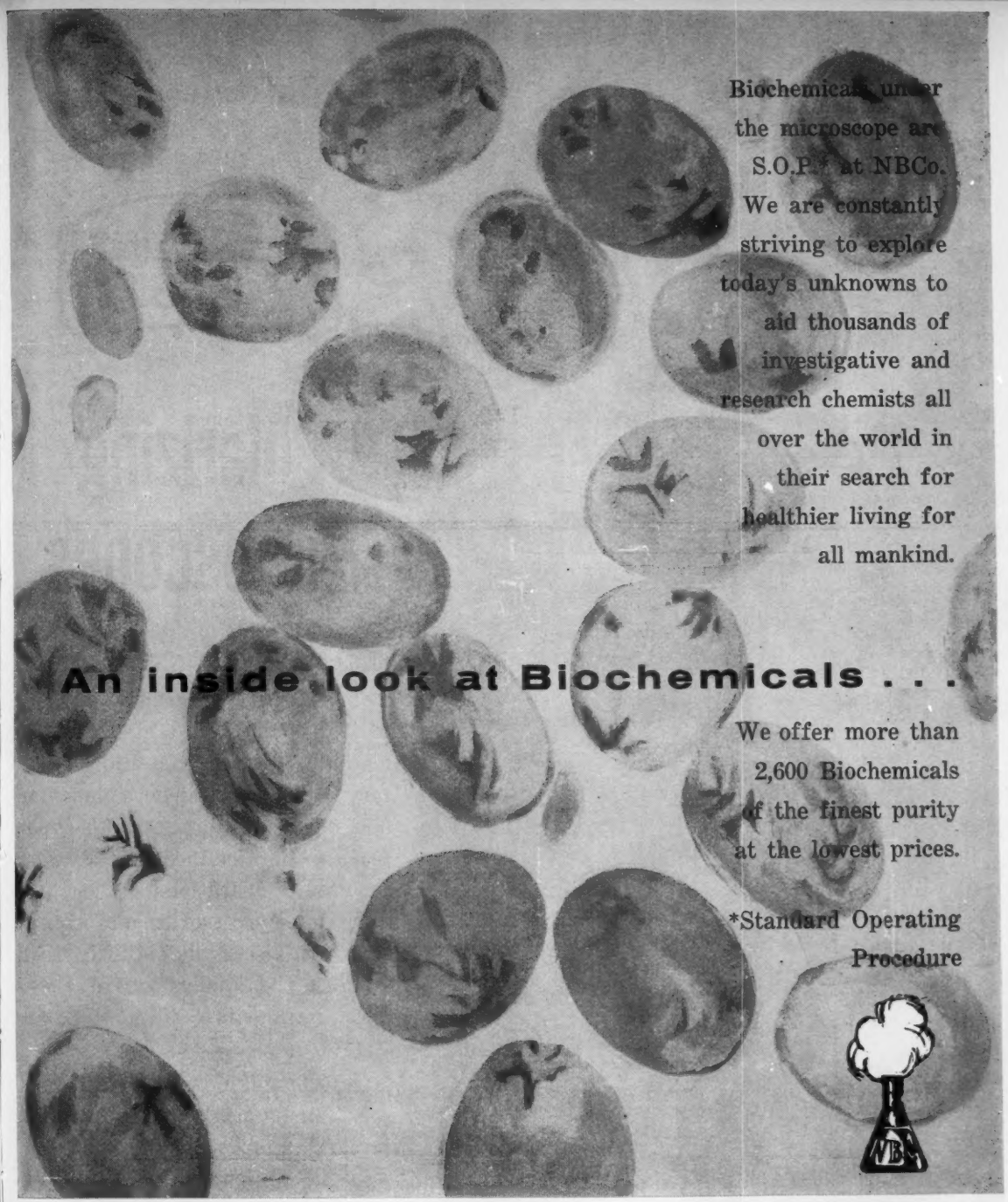

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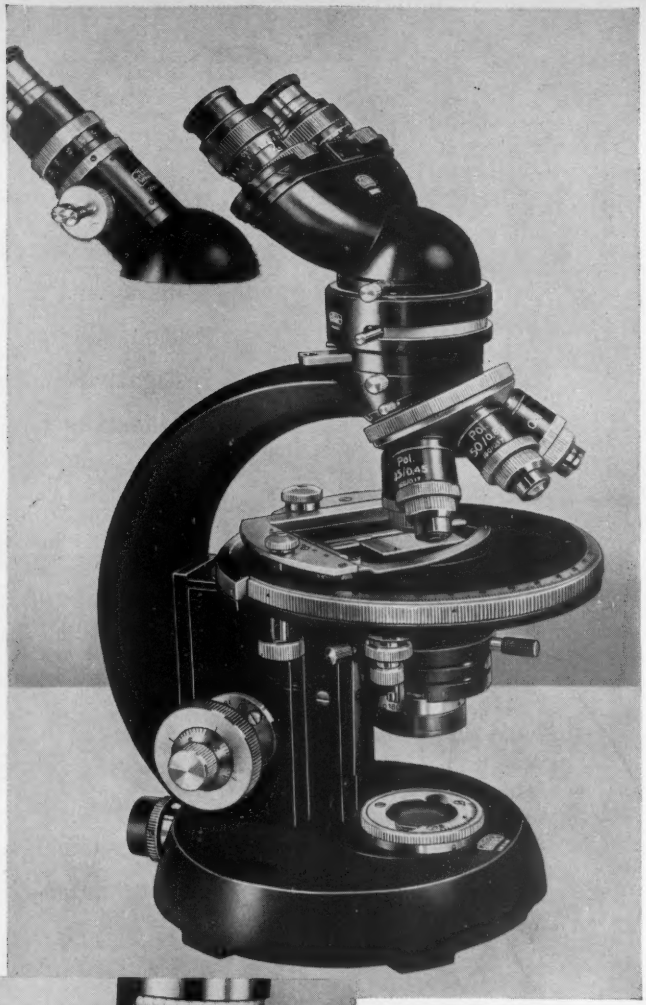

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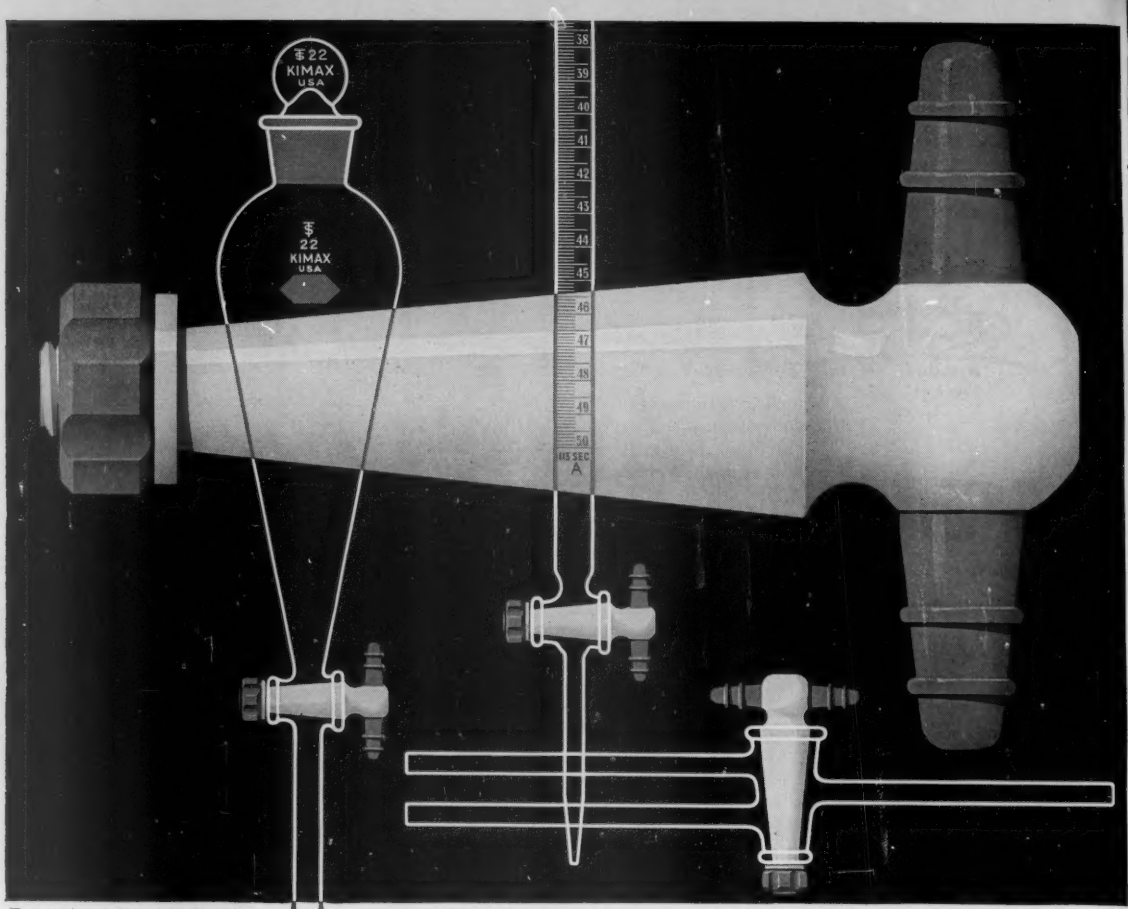

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Reverse in Geneva

One effect of the summit collapse in Paris on the negotiations for a ban on nuclear weapons testing in Geneva was the reversal by the Soviet delegation of its decision to go along with the United States proposal that more research be conducted on ways to control small underground explosions. Soviet scientists had said that although the Soviet Union would not conduct experiments with nuclear devices, there would be a three-year program using chemical explosives. But about three weeks after the summit collapse, the Soviet delegation announced at the political level that there would be no tests of any kind in the Soviet Union and that Soviet scientists had not been empowered to speak in the name of the Soviet government.

In seeking better ways to detect small underground tests, United States scientists had, for their part, proposed exploding a series of nuclear devices. Soviet scientists had objected to some experiments in the American series, but they agreed to others on condition that adequate safeguards be provided to assure them that no results of military value could be gained along with results of use in improving the design of a control system.

By way of assurance, the U.S. has subsequently offered to share all test results with the U.S.S.R. and to admit Soviet observers to the test site. The U.S. has also offered to put immediately on deposit, under international supervision, boxes containing all the bombs to be used in the research program, and so to demonstrate that results from devices exploded earlier in the series could not be used as the basis for design of the devices exploded later. The Soviets have demanded, however, precisely what the Americans have not offered and what is against the law barring the disclosure of nuclear secrets—namely, a look at the devices themselves. There may be ways for the United States to escape from this predicament, but any proposal is easily countered by a person determined to offer objections. A proposal to explode Soviet bombs instead of American bombs, for example, could be construed by Soviet negotiators as a thinly disguised plan to spy out the characteristics of Soviet weapons.

The U.S. is now mounting an effort to determine just what Soviet intentions are on a test ban. It is important, of course, to see whether we actually can specify safeguards for our research program that will prove acceptable to the U.S.S.R., but the course of the technical talks is ample evidence that the Soviets are as informed as we are about the limits of the present technology of test controls and about the advantages and disadvantages of a research program. Reversal of the Soviet stand on its own research program, with no explanation offered, suggests that the Soviets have found in their demand for assurances an opportunity to stall negotiations while making it look as if it is we who are doing the stalling.

Accordingly, in the new effort to determine Soviet intentions, the United States delegation should focus its efforts on the political level. We should push for answers on such familiar matters as the number of on-site inspections the Soviets will permit and the nationality of the staff at the control posts—questions which the summit meeting was supposed to have answered. It is on the political level that the more fundamental differences lie. And it is here that we are in the better position.—J.T.

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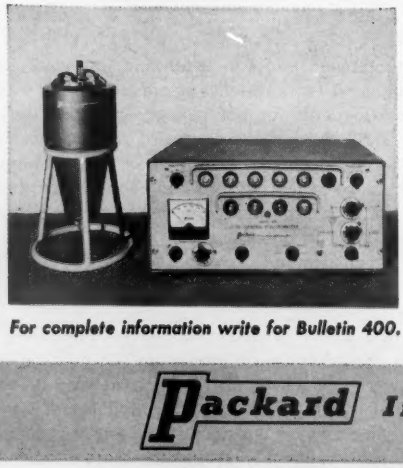

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Normal Sources of Pathological Behavior

Animal experimentation shows that certain combinations of adaptive responses can result in nonadaptive behavior.

Murray Sidman

While they were once held to be incompatible, clinical and experimental medicine have lived together so long and so harmoniously that they are now recognized to be at least common-law partners. The bond that united them was the thesis, ably demonstrated and eloquently expounded by Claude Bernard, that pathological states may be manifestations of normal processes—normal, not according to any statistical criterion, but in the sense that they carry on a lawful existence independently of their pathological manifestations. The study of disease and the study of normal physiological functions have thus come together within a deterministic framework. Clinical medicine has developed a truly experimental foundation and looks to basic science for future progress.

With respect to pathologies of behavior, however, clinical practice and laboratory experimentation have yet to achieve a satisfactory working partnership. The origin of pathology in normal behavioral processes is beginning to be recognized (see, for example, *1*), but in a not very large segment of current experimental or clinical practice. Experimental and clinical psychologists alike seem to equate the two terms *abnormal* and *disorderly*. Thus, when an experimenter isolates a lawful behavioral phenomenon, he is likely to consider that its very lawfulness removes it from the realm of clinical interest. Similarly, the clinician who does venture into the laboratory will, more often than not, try to demonstrate the absence of lawfulness in some behavioral phenomenon. Neither worker seems to give much thought to the possibility that maladaptive behavior can result from quantitative and qualitative combinations of processes which are themselves intrinsically orderly, strictly determined, and normal in origin.

I shall try to demonstrate a case of this sort here. The clinician may not have available, when I have finished, any new diagnostic or therapeutic tool, but if he can relate the events in my story to these introductory remarks, we may move somewhat closer to an experimental foundation for clinical psychology. The point of view must generate a practice before it can show practical results. The course that has proven so fruitful in medicine should also yield rich dividends in psychology.

Estes-Skinner Experiment

In a paper on "Some quantitative properties of anxiety" (*2*), published in 1941, W. K. Estes and B. F. Skinner described the changes produced in the lever-pressing activities of a rat by the sounding of a tone and administration of an electric shock when the tone ended. Observation by Estes and Skinner of the world about them—clinical observation, so to speak—had led these investigators to suspect that the term *anxiety* was often applied to behavior occurring during sequences of events similar to the tone and shock. The authors selected the laboratory rat as the subject for their experiment; but it was assumed that under similar circumstances other animals would behave in a similar fashion, and this assumption has subsequently been confirmed to a remarkable extent.

In the experiment, a rat is placed in a small chamber. The rat is first trained to press a lever projecting from the wall; as reinforcement it gets small food pellets from a tray underneath the lever (see Fig. 1). Next, the mechanism connecting the lever to the food-delivery system is scheduled so that lever pressing produces food only once every 4 minutes. As established by earlier experiments (*3*), on this schedule, a hungry rat will press the lever at a fairly steady rate.

The investigation proper is now begun, with experimental sessions lasting 1 hour. During each session the tone is sounded (through a phone in the chamber) once or twice, for 3 minutes at a time, and at the termination of the tone the rat receives a brief, unavoidable shock through the grill on which it stands. The food-reinforcement schedule remains in effect at all times, including the period when the tone is sounding. After a number of sessions the effect of the tone is to diminish greatly the rate at which the rat presses the lever. This phenomenon is called conditioned suppression.

We repeated the Estes-Skinner experiment in our laboratory, using a rhesus monkey as the subject and making the food pellets available at irregular intervals. Again, the result was conditioned suppression. As the record reproduced in Fig. 2 shows, when the

The author is affiliated with the Walter Reed Army Institute of Research, Washington, D.C. This article is adapted from a lecture presented 1 April 1959 at a symposium on the experimental foundations of clinical psychology, held at the University of Virginia School of Medicine.

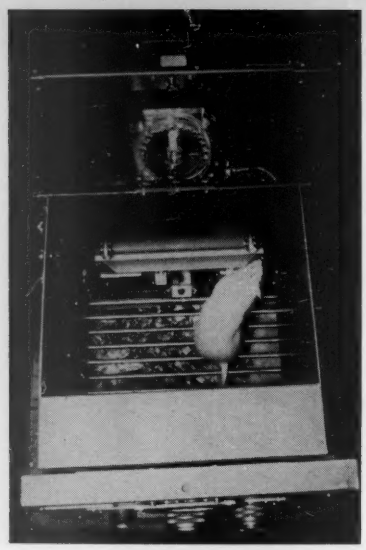

Fig. 1. An experimental chamber similar to the one used by Estes and Skinner.

tone sounded the monkey pressed the lever at a much slower rate. After the shock was administered, the monkey pressed the lever at its usual rate.

The disruption of ongoing activity is quantitatively reproducible for the same animal, for animals of the same or different species, and for animals in tests of behavior other than lever pressing. Yet this lawful phenomenon has pathological characteristics. As the animal reduces the rate at which it presses the lever, there is a resultant loss of food; this response would seem to have no adaptive value. Instead of pressing the lever, the animal displays other forms of behavior, which may range from complete immobility to agitated, intense, and apparently aimless locomotor activity, all accompanied by signs of autonomic upset.

Applications of the Estes-Skinner Technique

The small perturbation in the cumulative response curve of Fig. 2 may not correspond to one's favorite definition of anxiety. As Schoenfeld has noted, ". . . anxiety in its multifarious nonoperational meanings is a perfectly bad word. . . ." (*4*). But that is not the issue here. The fact remains that we have a simple technique for producing a profound change in an organism's behavior, a change that appears to be characteristic of a pathological condition. The simplicity of the manipulation should not deceive us. Whenever a simple operation is found to exert a powerful behavioral effect, we may suspect that the phenomenon can be widely generalized. In the present case, furthermore, the simple operation of exposing an organism to the stimulus-shock sequence leads to behavioral consequences of an exquisite complexity.

After it was first described, in 1941, the Estes-Skinner experiment received almost no experimental attention for a period of 10 years. It was finally resurrected by Hunt and Brady (*5*), who, with their collaborators, made it the basis of a productive research program. At first Hunt and Brady were interested in conditioned suppression not so much for its own sake as for its potentialities as a tool in studying other things. Their initial investigations, for example, dealt with electroconvulsive therapy. After their subjects, white rats, had developed a full-blown conditioned suppression, ceasing to press a lever when they heard the clicking noise which preceded shock, they were given a series of electroconvulsive "treatments." On being returned to the experimental chamber they no longer reacted to the clicker. Instead of being disrupted, the animals behaved normally during the warning stimulus. Electroconvulsive shock had "cured" them of their anxiety.

A long and revealing series of experiments followed, elucidating additional aspects of electroconvulsive shock treatment (*6*). In these experiments, Brady and Hunt employed conditioned suppression in much the same way as the physiologist, for example, utilizes the techniques of chemistry to investigate metabolic processes. Little more was learned of the behavioral processes themselves. But as additional uses were developed for the Estes-Skinner technique, greater attention was given to the behavioral processes, the initially silent partner in this study of a "cure." It is instructive both for the experimentalist and the clinician to follow at least part of the course of this development.

Another problem to which the suppression technique was applied was that of the effects of certain types of damage to the central nervous system. For example, if a clicking noise of mild intensity is sounded while a thirsty rat is pressing a lever to obtain occasional small drops of water, and an unavoidable shock is delivered to the rat when the clicker stops, the animal will eventually cease pressing the lever while the clicker is on. We then make a surgical lesion in the rat's septal forebrain region. When placed in the experimental situation again after it has recovered from the operation, the rat presses the lever for water in the same way as it did before. But when the clicker sounds, the animal does not reduce its rate of lever pressing to the degree that it did prior to the operation. The septal lesion tends to attenuate the conditioned suppression in much the same way as electroconvulsive shock does (*7*).

Certain drugs also change the reaction of both monkeys and rats to the warning clicker. Animals given reserpine over a long period, for example, gradually resume their normal rate of lever pressing during clicker stimuli which precede electric shock (*8*, *9*). Like electroconvulsive therapy, reserpine apparently cures their disturbed behavior.

There is a similar effect when rats and monkeys are rewarded for lever pressing not by food but by intracranial electrical stimulation via permanently

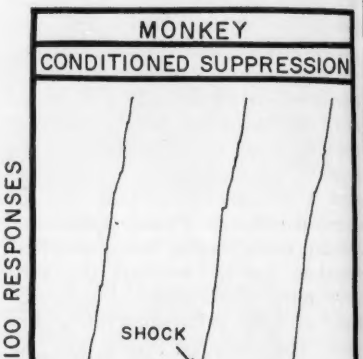

Fig. 2. An illustration of the Estes-Skinner conditioned suppression phenomenon. Responses are recorded cumulatively, with the pen automatically resetting to the base line after every 450 responses. The introduction of the clicking noise is indicated by the slight oblique downward displacement of the pen at the first arrow. The shock, which immediately follows termination of the tone, occurs at the point where the pen displacement is rectified, indicated by the second arrow.

implanted electrodes. (Intracranial electrical stimulation seems to function as a reward for the animal. The basic observation is that the animal will work for electrical stimulation in certain brain areas just as it will work for food; it is not necessary to assume that the animal derives pleasure from the electrical stimulus.) But, whereas an animal working for food will cease its lever-pressing activities after a series of clicker-shock experiences, the same animal, when working for electrical stimulation of the brain, will continue to press the lever while the clicker sounds (*10*). Though both function as rewards, brain stimulation and food cause animals to react differently in an anxiety situation.

As far as the behavioral processes were concerned, we had not, at this point, progressed much beyond the original Estes-Skinner phenomenon. But the behavioral perturbation that often accompanies the warning stimulus shows both an order and a complexity which help us to understand behavioral pathology. One more application of the technique will show how we were led to consider conditioned suppression as of interest in its own right.

A research program was under way to test the notion that behavioral phenomena and the functioning of the pituitary-adrenocortical system are correlated. On the endocrine side, John Mason and his collaborators had developed a reliable technique for measuring blood levels of 17-hydroxycorticosteroids in monkeys (*11*) and were engaged in a series of studies to determine the anatomical and physiological properties of the system. On the psychological side, we set out to determine whether this system could be activated by behavioral methods. One of our successful ventures involved the Estes-Skinner technique. When blood samples were taken from monkeys before and after their exposure to the Estes-Skinner procedure we found that a marked elevation had occurred in the plasma level of 17-hydroxycorticosteroids (*12*). But in this application we were forced to depart, to a certain extent, from the original procedure. All of the departures were dictated by practical necessity, but they had revealing systematic consequences.

A first change was required when we found we could not use the laboratory rat. Nearly all previous work on conditioned suppression had been performed with this useful animal, but unfortunately it did not have a sufficient quantity of blood for repeated steroid measurements. We therefore turned to the monkey. In the initial experiments with the monkey, as in previous work, we combined stimulus and shock only once or twice during any given session. While our subjects did display elevation of steroid levels in these first tests, the elevations were not as consistent as we felt requisite for further investigation. Following a procedure developed by Azrin (*13*) we therefore subjected the monkeys to a large number of stimulus-shock sequences during each experimental session and, when this procedure showed signs of success, went on to a schedule in which stimuli of 5 minutes' duration were programmed every 10 minutes. Findings under these conditions gave us clean and reproducible base lines; the animals ceased to respond during each stimulus but began to press the lever again almost immediately after each shock (see Fig. 3). Correlated with this relatively stable behavior was a reliable and large elevation in steroid level.

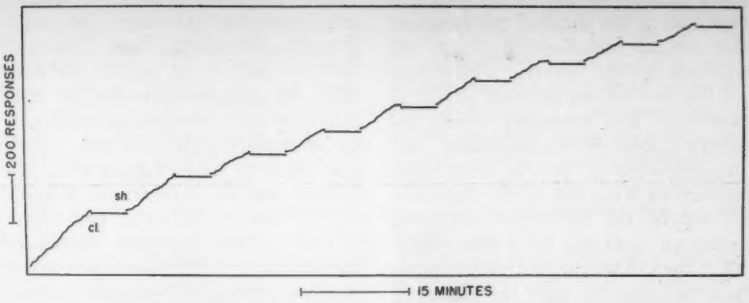

Fig. 3. Cumulative record of lever pressing. Five-minute periods of clicking alternated with 5-minute periods of silence. The introduction of the clicking noise is indicated by the slight oblique downward displacement of the pen and is marked *Cl* at the first presentation. The shock occurs at the point where the pen displacement is rectified. The first shock is marked *Sh*.

Was this, however, a pure example of behavioral stress, or was the shock a necessary part of the picture? In order to answer this question we attempted to run test sessions in which we gave the subjects the warning stimuli as usual but no shock. Would the stimuli alone, without shock, increase the output of 17-hydroxycorticosteroids? The procedure, unfortunately, did not allow us to answer this question. When the shock was discontinued the animals pressed the lever at their usual high rate during the warning stimuli. There was no longer any conditioned suppression or any rise in steroid level.

We solved the problem by following the stimuli with shock only intermittently. The monkeys received shock after 25 percent of the warning stimuli in each session; the remaining stimuli were not followed by shock. Then, in subsequent test sessions, we again presented the warning stimuli to the subjects without shock. Consistent with the general principle that intermittent reinforcement prolongs the process of extinction was the finding that a diminished rate of lever pressing and a rise in steroid level were elicited by the warning stimuli alone.

But the story is not a continuous chronicle of success. It was necessary to keep each monkey working consistently for extended periods of time, pressing the lever steadily but ceasing to press when the clicker sounded. Stable behavior was required if further investigation was to continue. But when the modified Estes-Skinner procedure had been in effect for some time, the monkey's behavior began to deteriorate. An example may be seen in Fig. 4. The contrast with the regularity shown in Fig. 3 is striking. The subject now presses the lever at a very uneven rate between stimuli, with suppression sometimes continuing even after the clicker is silent. Occasionally, as at the points marked *a*, the animal ceases lever pressing immediately before the noise is introduced. During the stimulus period there is evidence of temporal discrimination: as indicated at *b*, the animal continues pressing the lever during the early minutes of the stimulus; then suppression occurs. Another strange phenomenon is indicated at *c*: after a period of suppression, the monkey begins to press the lever at a low, steady rate which continues until it receives the shock.

This breakdown of the behavioral

base line is the kind of phenomenon that suggests pathology. We may well conclude that the behavior is pathological but not on the grounds that it is disorderly. The anomalies which began to appear in the behavioral records appeared to be disorderly only because we were not at the time able to identify the controlling variables. Unless we could gain some understanding of the behavioral processes at work here, we could not apply the technique to the problem of behavior-endocrine interaction. Out of our investigations, then, there emerged a new appreciation of the behavioral complexities with which we had been working. Our first attempts to unravel these complexities seemed to multiply them, producing behavior which, if not pathological, was certainly bizarre, but we were eventually able to show that even the most bizarre performances were under the control of orderly and manipulable factors. In no sense did they represent deviations from lawful behavior.

Some Fruits of Basic Research

Reinforcement cost. From the several changes we had made in the basic Estes-Skinner procedure, we selected two temporal variables for further study. These were (i) the period during which the clicker sounded and (ii) the period during which the clicker was silent. Using white rats as subjects, we systematically manipulated these two periods (*14*). Much to our surprise, we found that such manipulation reproduced several of the phenomena shown in Fig. 4. The most general finding, however, was that both temporal variables controlled the degree of conditioned suppression, but only when they were considered in relation to each other. An animal would press the lever at a very slow rate during a clicking period of a given duration only if there were relatively long intervals of silence between clicking periods. An example may be seen in the upper two curves of Fig. 5. With 24-minute periods of silence, the subject makes only a few responses during a 6-minute clicking period (at *a*). Suppression is nearly complete. But when the periods of silence are reduced to 2 minutes, the animal responds considerably more often during a 6-minute clicking period (at *b*). Similarly, as may be seen further down in Fig. 5, with 2-minute periods of silence and ½-minute clicking periods the subject again fails to respond (at *c*). Complete suppression reappears.

We now knew how to maintain a stable conditioned suppression in the subjects of our steroid studies and in other applications of the Estes-Skinner technique, but the real plum was another observation which illuminated the process through which the animals' behavior was controlled by the temporal variables. The subjects of the experiment had been deprived of water, and the reinforcement for lever pressing was a small drop of water. We noticed that the number of drops of water the animals received was relatively constant, about 90 percent of the maximum possible, regardless of the effectiveness of the clicker in diminishing the rate at which they pressed the lever. But, it will be recalled, conditioned suppression causes the animal to miss reinforcements, for it is only by pressing the lever that a drink can be obtained. In other words, the animals displayed conditioned suppression only to the extent that they could do so without missing more than 10 percent of their drinks. Thus, if the clicking period was short relative to the period of silence, the animal could cease pressing the lever during the clicking and still miss relatively few drinks in the course of an experimental session. On the other hand, if the clicking period was relatively long, complete cessation of lever pressing would cause the animal to lose most of the available drinks.

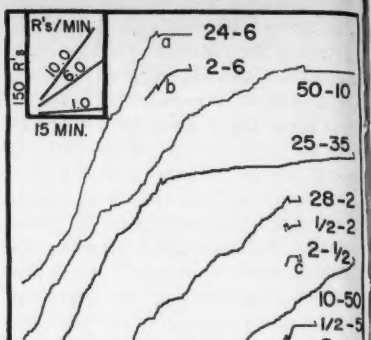

Fig. 5. Sample cumulative records made in tests with periods of clicking and of silence of various lengths. The first number of each pair designates the number of minutes of silence; the second, the number of minutes of clicking. The oblique downward displacement of each curve indicates the point at which clicking was introduced.

Although more work must be done before the phenomenon is entirely clear, we might say, at this point, that the animals manifest anxiety only to the extent that they can afford to do so in terms of reinforcement cost.

Aversive interactions. A unique feature of the Estes-Skinner technique is its use of changes in the organism's ongoing behavior to measure the consequences of an independent but concurrent experimental operation. The effect of the warning stimulus may be described, in most general terms, as a disturbance in the pattern of behavior in progress at the time the stimulus appears. As we have seen, one form of disturbance is complete cessation of the behavior. It seems reasonable to suppose that the variables which control the base-line activity also have a role in determining the effect of the

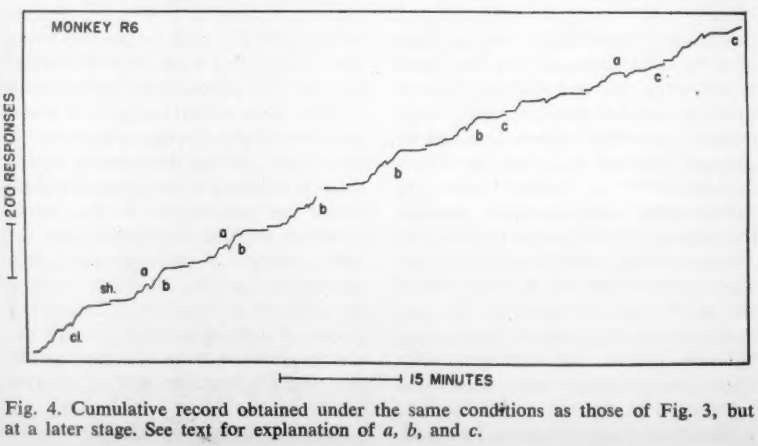

Fig. 4. Cumulative record obtained under the same conditions as those of Fig. 3, but at a later stage. See text for explanation of *a*, *b*, and *c*.

warning stimulus. In one of the few attempts yet made to verify this supposition, Brady found that rats reacted to a warning stimulus with conditioned suppression even when they received their water in accordance with several different types of schedules. However, when the stimulus was then presented without any shock, it was found that the type of reinforcement schedule did influence the length of time it took the animals to resume their normal rate of lever pressing (*15*). The variables controlling the normal behavior pattern, then, did affect the temporal course of the animals' "rehabilitation."

All demonstrations of conditioned suppression up to this point had one feature in common. Food or water reinforcement was always used to maintain the subjects' base-line behavior. What would be the consequence of presenting the clicker-shock sequences while the subjects were pressing a lever to avoid electric shocks?

In the experiments described below, monkeys were the subjects, and the experimental space was similar to, but larger than, that previously described for the rat (see Fig. 1). We first conditioned the monkeys to press a lever by the simple expedient of giving them a brief shock whenever 20 seconds elapsed without a lever depression. Each time they pressed the lever they postponed the shock for 20 seconds (*16*). After the animals had settled down to a relatively stable rate of avoidance responding we introduced the clicker and unavoidable shock sequence, using the earlier schedule of 5-minute clicking periods alternating with 5-minute periods of silence.

The immediate result was that the animals pressed the lever at approximately three times their normal rate, both when the clicker was on and when it was silent. In fact, they responded sufficiently often to avoid all avoidable shocks; the only shocks they received were the unavoidable ones (*17*). The monkeys then gradually slowed down to their normal rate of lever pressing. But they returned to their normal rates more rapidly when the clicker was silent than when it was sounding. There was, therefore, an intermediate phase in which they pressed the lever at a higher rate during the clicking periods than during the periods of silence. This reversal of the Estes-Skinner observation caught our interest.

We eliminated the avoidable shocks but continued to administer the unavoidable ones. The monkeys ceased lever pressing, as was to have been expected, during the periods of silence. But for a long time they persisted in lever pressing during the clicking periods. Figure 6 shows the striking reversal of the usual conditioned suppression; the animal practically never responds during periods of silence or during the initial minutes of the clicking periods. But as the time approaches for shock, the monkey begins to press the lever rapidly and continues until it receives the shock. Immediately after the shock, it again ceases pressing, and another cycle begins. This phenomenon is called conditioned facilitation.

Does conditioned facilitation during the clicking period represent a breakdown of the lawfulness to which we have become accustomed in our experience with the Estes-Skinner technique? From an adaptive point of view, the facilitation of lever pressing makes no more sense than does suppression. The shock is inevitable, and the animal's high response rate during the stimulus represents only so much wasted energy. It would take very little stretching of the imagination to class this behavior as pathological. Yet, as we shall see, it results from normal processes at work in a slightly unusual setting.

When an animal that is pressing a lever for food is first exposed to the clicker-shock sequence it may initially cease pressing both when the clicker is on and when it is silent, even though it receives shock only while the clicker is on. This may be thought of as a generalized effect of unavoidable shock. A corresponding generalized effect, an over-all increase in response rate, is initially observed when the lever pressing has served to postpone shock. In their first stages, then, the two effects are opposite in direction but similar, perhaps, in origin.

A second stage occurs when the initially generalized effect of shock is channeled into the clicking period. A monkey working for food returns to its normal rate of lever pressing during periods of silence but continues to display suppression during the clicking periods. Similarly, a monkey pressing the lever to avoid shock returns to its normal response rate during periods of silence but continues to display conditioned facilitation during the clicking periods.

If this were the whole story, both the suppression and the facilitation might well be construed as emotional reactions to the unavoidable shock, the precise form of the reaction depending upon the subject's past experience of shock. Perhaps some such formulation could encompass the observations thus far discussed. But one additional observation does not fit. Under appropriate conditions, some of which I discussed above, the conditioned suppression becomes fixed at the second stage. The conditioned facilitation, on the other hand, goes through a third phase. It disappears. It is really a transitory phenomenon, though its life span and magnitude are sufficiently great to merit

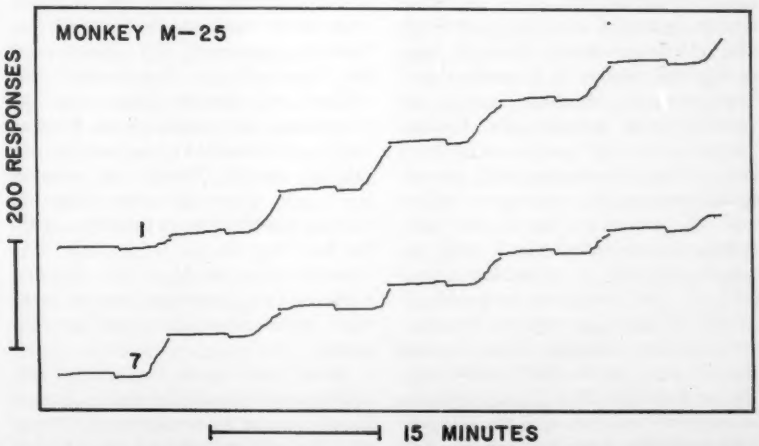

Fig. 6. Response facilitation during the period of clicking prior to shock. The introduction of clicking is marked by the downward displacements in the record. The lower curve is actually continuous with the upper one, but has been displaced for compact presentation. The first and seventh clicking periods of the session are labeled.

both experimental and clinical attention. The impermanence of the facilitation places it in a different category from the suppression.

We can, however, hold on to the notion that avoidance conditioning is a prerequisite if the animal's rate of lever pressing is to increase, rather than decrease, during the clicking periods. If an organism has learned a successful shock-avoidance response, what is more appropriate, in a situation in which shocks occur, than that very response? The source of control of the facilitation may be the normal processes which govern the subject's previously acquired avoidance behavior. But we are still faced with the fact that the animal actually cannot avoid the shocks. On many occasions the monkey is almost immediately punished by the shock for its increased rate of lever pressing during the clicking period. How can we reconcile the inevitability of the shock with an explanation based upon the normal and orderly processes that underlie successful avoidance behavior?

The difficulty is more apparent than real, but I have not simply been building up a straw man. I have examined the problem in detail because its simple solution reveals a form of behavioral control which, because of its subtlety, one might easily overlook. In the actual relations between the shock and facilitated lever pressing two facts are to be noted. First, the monkey receives only one shock during each 5-minute clicking period; hence, only an extremely small proportion of its lever-pressing responses are actually punished. From the subject's point of view, lever pressing, by which it has in the past effectively avoided shocks, seems to remain largely successful. Avoidance of shock still reinforces lever pressing, even though the relation is a spurious one. The monkey's behavior during the clicking period is nonadaptive because the rules of the environment have changed and the changes have not yet elicited appropriate response modification. The occasional shocks only serve as false discriminative cues to keep the animal behaving in a fashion appropriate to the former circumstances.

The second point concerns the temporal relation between response and shock implied by the term *punishment*. The time interval that elapses between an unavoidable shock and the immediately preceding lever response is variable. In other respects the situation is exactly the same as that during the original avoidance conditioning. Originally, the monkey postponed the shock for 20 seconds each time it pressed the lever. Now, the shock sometimes occurs 2 seconds after a response, sometimes 200 seconds after, sometimes 20 seconds after, and so on. We have already demonstrated that animals will continue to avoid shock successfully when we systematically vary the amount of time they postpone shock with each lever press (*18*). In the present case, the variation is governed not by the experimenter but by the vagaries of the subject's own lever-pressing. The contingencies are spurious, but the control they exercise is real.

In its late stages at least, facilitation during the clicking period may properly be understood as avoidance behavior which the monkey continues to manifest because of a combination of historically real and currently adventitious contingencies. It is not nearly as general a phenomenon as conditioned suppression; it requires an organism with a particular type of behavioral history, and it requires a unique set of current circumstances which serve to perpetuate the processes that stem from this history even after they are no longer relevant to the demands of the environment.

Direct manipulation of the monkey's behavioral history in a subsequent experiment effectively demonstrated its relevance. In the first phase of the experiment we conditioned the monkey to press the lever by reinforcing it with food. Once the monkey was pressing the lever at a steady rate, we introduced the clicker-shock sequences until a conditioned suppression developed during the clicking period.

The next step was to add an avoidance component to the subject's behavioral repertoire. We disconnected the food-delivery mechanism, the clicker, and the mechanism that delivered the unavoidable shock. Pressing the lever now served to postpone shocks for 20 seconds. Finally, the monkey was again given the opportunity to procure food by pressing the lever without receiving shocks at any time. The variables were the same as in the first phase of the experiment, but the monkey's experience with shock was different.

When we again introduced the clicker-shock sequences the avoidance history proved to be dominant. Whenever the clicker sounded the monkey pressed the lever at a much higher rate, even though it was working for food (*19*). By interpolating a period of avoidance conditioning between the two stages of the Estes-Skinner procedure we had changed conditioned suppression to facilitation. The conclusion seems inescapable that the facilitation represents a form of avoidance behavior, irrational perhaps, certainly ineffective, but nonetheless derived from identifiable and orderly sources of control.

A final set of experiments provided us with an unexpected view of the twisted fashion in which normal behavioral processes can manifest themselves (*20*). Our initial aim was simple enough. We had seen that the effect on a given response of a stimulus that precedes a shock will depend upon the history of that response. If we now select for simultaneous observation two different responses, one of which the monkey has used to procure food and the other of which it has used to avoid shock, will a warning stimulus generate two concurrent but opposite reactions in a single animal?

To answer this question, we made two opportunities simultaneously available to the monkey. Hanging down from the ceiling of the chamber was a chain, and pulling this chain occasionally paid off with food, while pressing a lever mounted on a wall of the chamber postponed shocks for 20 seconds. Both the food schedule and the avoidance program were in effect concurrently. The monkey adjusted appropriately to the contingencies, sometimes pulling the chain and sometimes pressing the lever.

Clicker-shock sequences were then introduced, and the avoidable shocks—shocks previously governed by lever pressing—were eliminated. The only shocks the monkey received were the unavoidable ones that followed the clicking periods. In line with previous findings, we expected the monkey, during the clicking period, simultaneously to reduce its rate of chain pulling, displaying conditioned suppression, and to increase its rate of lever pressing, displaying conditioned facilitation.

In fact, the animal's rates of response for both chain pulling and lever pressing rose during the clicking periods. There was no evidence of suppression. Rates for both types of response were relatively low during periods of silence. This was the pattern for a response with an avoidance history, yet we had not provided the food-reinforced response with such a history. Did this mean that the lawfulness revealed in the prior experiments is missing when

two response systems within a single organism are simultaneously exposed to the Estes-Skinner procedure? Such an interpretation would be consistent with the classical view of behavior pathology, and, if we had accepted it, we should have stopped work at that point. As it turned out, however, our resistance to the classical view permitted us to round out the story. The final experiments not only revealed orderly processes but, in addition, permitted us to explain some material classically considered pathological.

We found that our monkey's two concurrent responses were not entirely independent. For example, we returned the animal to the initial training procedure in which it produced food occasionally by pulling the chain and avoided shock by pressing the lever. We then disconnected the feeding mechanism. But even though no food was forthcoming, the animal continued to pull the chain at a relatively high rate. The monkey stopped pulling the chain only after we had also disconnected the shock, thereby causing it to stop pressing the lever. Only with the cessation of avoidance behavior did the monkey cease pulling the chain. It seemed clear that the food-reinforced behavior was being controlled in some way by the avoidance contingency, even though no such control was demanded by the experimental arrangements. The process through which this control developed is a most fascinating one, for it takes us, in a manner of speaking, into the "inner life" of our animal subjects.

We have already described the facilitating effect of the warning clicker as a case of adventitiously reinforced, or "superstitious," avoidance behavior. (I use the term *superstitious* in the operational sense in which Skinner uses it, to describe a situation in which a particular response is correlated only by chance with a reinforcing state of affairs (*21*). Even though the behavior may not actually produce the reinforcement, and though the correlation may not even be advantageous to the organism, the reinforcing effect is not thereby weakened.) The monkey's behavior during the stimulus period is reinforced by the seeming avoidance of shocks. We, as experimenters, know that the animal would not have received shocks anyway, even if it had not pressed the lever, but our monkey is a prisoner of its behavioral history.

The subject was free to make the two possible responses in any sequence. If it frequently pulled the chain and then pressed the lever, the pattern might become established as superstitious avoidance. If the monkey could speak, it might well tell us that it was avoiding the shock by first pulling the chain and then pressing the lever. The chain-pulling response, though reinforced with food, might also develop an avoidance component, which would explain the increased rate of chain pulling during a clicking period.

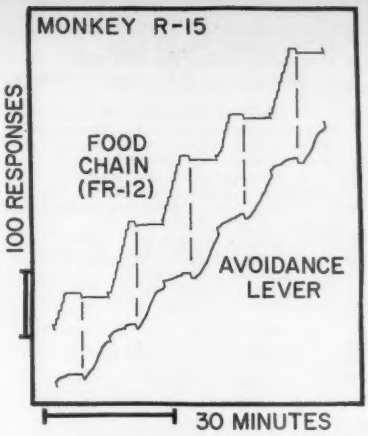

Fig. 7. Concurrent cumulative records for chain pulling and lever pressing. The portions of the records displaced obliquely downward denote clicking periods that preceded shock. The broken lines connect temporally corresponding points (introduction of clicking) on each curve.

Being both unable and unwilling to rely upon the verbal report of our subject, we made an experimental search for adventitious reinforcement processes. We found that the monkey was actually making the two responses in sequences of the sort that would favor the development of a superstitious avoidance pattern. There was only a low probability that the animal would pull the chain twice without pressing the lever in between; the vast majority of chain-pulls were followed by lever presses. There was abundant opportunity for chain pulling to be correlated accidentally with the avoidance of shock.

There remained only the task of breaking up the alternation pattern and thereby eliminating the avoidance component of the food-reinforced response. This was accomplished by utilizing a bit of behavioral technology. Up till now only an occasional chain-pull had paid off on a temporal schedule. The schedule was changed so that the animal had to pull the chain a fixed number of times to procure food. Since such a schedule favors the reinforcement of rapid bursts of responses (*3, 22*), there should be a tendency for the monkey to make several chain-pulling responses in succession before switching to the lever.

The new schedule accomplished its purpose. The likelihood that the animal would press the lever after only a single chain pull decreased markedly. Instead, the monkey showed a marked tendency to pull the chain several times before pressing the lever. The typical pattern of bursts and pauses may be seen in the upper curve of Fig. 7.

Figure 7 also indicates that we eliminated the superstitious avoidance component of the food-reinforced response. For now, when we introduce the clicker-shock sequence there is no increase in the monkey's rate of chain pulling. The clicking simultaneously suppresses the food-reinforced response and facilitates the avoidance response. Although both forms of response exist simultaneously in the same organism, each is affected by the clicking according to its own history.

It has been necessary to report the experiments in such detail in order to illustrate the complete normality of the processes underlying our initial finding of facilitation in both responses. In tracking these processes down, we have seen how they may act to produce some bizarre manifestations. Thus, behavior which has no real connection with the shock must nevertheless be diagnosed as avoidance behavior, spuriously maintained as part of an avoidance pattern. Then, this behavior, already under spurious control, perpetuates itself during the clicking period by seemingly permitting the animal to avoid shocks that would not have occurred anyway. Such behavior may be called "second-order superstition." If this is "sick" behavior, the processes that generate and maintain it are healthy enough.

Whether these particular experimental phenomena are indeed basic to the understanding, diagnosis, and treatment of clinically observed behavior pathology remains an open question. But there can be no doubt that such experimental manipulation has the necessary power and subtlety to uncover processes relevant to clinical observations. The clinical psychologist need no longer seek his experimental foundations among demonstrations of behavioral chaos. The experimentalist, too, would do well to cultivate an interest in pathology as a source of insight into normal behavioral processes (*23*).

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23. I am indebted to Martha Crossen for her constructive suggestions during the preparation of this article.

Science and Human Welfare

The AAAS Committee on Science in the Promotion of Human Welfare states the issues and calls for action.

For nearly two decades, scientists have viewed with growing concern the troublesome events that have been evoked by the interaction between scientific progress and public affairs. With each advance in our knowledge of nature, science adds to the already immense power that the social order exerts over human welfare. With each increment in power, the problem of directing its use toward beneficial ends becomes more complex, the consequences of failure more disastrous, and the time for decision more brief.

The problem is not new, either in the history of human affairs or of science. What is without past parallel is its urgency.

Four years ago, the report of the AAAS Interim Committee on the Social Aspects of Science (*1*) stated: "We are now in the midst of a new and unprecedented scientific revolution which promises to bring about profound changes in the condition of human life. The forces and processes now coming under human control are beginning to match in size and intensity those of nature itself, and our total environment is now subject to human influence. In this situation it becomes imperative to determine that these new powers shall be used for the maximum human good, for, if the benefits to be derived from them are great, the possibility of harm is correspondingly serious."

The Interim Committee also concluded that "there is an impending crisis in the relationships between science and American society. This crisis is being generated by a basic disparity. At a time when decisive economic, political, and social processes have become profoundly dependent on science, the discipline has failed to attain its appropriate place in the management of public affairs."

In the last few years the disparity between scientific progress and the resolution of the social issues which it has evoked has become even greater. What was once merely a minor gap now threatens to become a major discontinuity which may disrupt the history of man.

Recent events have lent substance to the conviction of our committee and of its antecedent groups—and we believe to that of scientists generally—that scientists bear a serious and immediate responsibility to help mediate the effects of scientific progress on human welfare, and that this obligation should be reflected in the program of the AAAS.

In the present report we endeavor to translate this conviction into action by suggesting a general approach and some specific procedures which may serve as a guide for the development of a AAAS program on the role of science in the promotion of human welfare.

Now, as in 1956, our premises are these (*2*):

1) We are witnessing an unprecedented growth in the scale and intensity of scientific work.

2) This growth has been stimulated by an intense demand for the practical products of research, especially for military and industrial use.

3) The public interest in, and understanding of, science is not commensurate with the importance that science has attained in our social structure. It cannot be said that society provides good conditions for the proper growth of science.

4) For reasons such as those just cited, science is experiencing a period of rapid but rather unbalanced growth. Basic research, which is the ultimate source of the practical results so much in demand, is poorly supported and, in the view of some observers, lacks vigor and quality.

5) The growth of science and the great enhancement of the degree of control which we now exert over nature have given rise to new social practices, of great scope and influence, which make use of new scientific knowledge. While this advance of science has greatly improved the condition of human life, it has also generated new hazards of unprecedented magnitude.

Members of the committee are Barry Commoner, Washington University, *chairman*; Robert B. Brode, University of California, Berkeley; Harrison Brown, California Institute of Technology; T. C. Byerly, Agricultural Research Service; Laurence K. Frank, 25 Clark Street, Belmont, Mass.; H. Jack Geiger, Harvard Medical School; Frank W. Notestein, Population Council, New York; Margaret Mead, American Museum of Natural History (ex officio Board representative); and Dael Wolfle, AAAS (ex officio).

An Estimate of the Present Situation

Since 1956 this general pattern has taken on some new features which concern us at this time.

1) The conscious exploitation of science for military advantage continues at an accelerating rate. But in recent years this process has merged with another, equally important trend: science is being pressed into the service of international politics. Scientific accomplishment per se has become an accepted—and at present dominant—factor of prestige among nations. The philosophy of "getting ahead of the Russians" (or Americans), which once referred only to military matters, now includes scientific achievements as well. This rivalry has strongly motivated the recent intensification of government support for scientific research.

2) The rapid emergence of political independence among the "underdeveloped" nations of the world, and their natural desire to exploit modern technology, has added to the importance of international exchange of scientific knowledge and personnel. Perhaps one reason for the rivalry for scientific preeminence among the more advanced nations is the expectation of political advantage from this exchange.

3) Certain recent scientific advances add directly to the ease with which our knowledge of nature can be applied to the control of human beings and of social organization. Development of new psychotomimetic drugs and psychological techniques have suggested, to some, effective means for controlling the behavior of social groups. Progress in the science of cybernetics and the development of automation techniques result in new capabilities for direct control of social and economic processes.

4) Despite some recent effort toward improvement, there is no reason to alter the earlier conclusion that our present social environment does not favor the development of an understanding of science, or of science's aims and needs. The increasingly spectacular practical achievements of science have only accentuated misconceptions about the relative significance, for the growth of science, of practical results and the advancement of basic knowledge. To many people physical science means nuclear energy and rockets. The public is sometimes led to expect that biological and medical research will conquer every human ailment—will overcome death. There is a tendency to equate scientific progress with a sum of money and a number of people. There is insufficient appreciation of the significance of basic research, or of the conditions in which it can flourish.

The situation appears to be this: We are witnessing an unprecedented and accelerating rate of growth in man's power over his environment. Science, the instrument which produces this power, is being consciously exploited for industrial, military, and political purposes. At the same time there is little recognition of the internal needs of science, or of its purposes as a discipline of the human mind.

In this situation it is inevitable that the inner strength of science should suffer, for what is essential to the proper growth of science is often in conflict with the conditions of its service to military and political affairs.

An important example of this effect is the matter of "competition." The military and political advantages, to a nation, of scientific progress within its own borders are self-evident. Yet, it is a truism—but nevertheless a vital one—that nature is the same everywhere, and that the study of nature is an activity of the whole human race. Any effort to divide science into fragments which are delimited by national boundaries, and dominated by a local social philosophy, will inevitably restrict the free discovery and communication of new knowledge that is the substance of scientific progress. A "nationalistic" science is an anachronism which cannot long continue without damage to science, and eventually to the nation.

What, then, is the scientist's responsibility to his own nation's scientific effort? Clearly, we need to understand that what science contributes to the national purpose is measured by what it adds to the sum of human knowledge; science serves the nation by serving humanity.

A further examination of the effects of the present social uses of science on life inside the house of science itself leads to even more disturbing conclusions. There is some evidence that the integrity of science is beginning to erode under the abrasive pressure of its close partnership with economic, social, and political affairs.

In recent controversies about fallout and the detection of nuclear explosions, partisanship on the part of some scientists for a particular political approach to the problem has been so intense in some instances as to cloud—at least in the public mind—the identity between science and an objective regard for the facts.

The grim international competition for "supremacy" in scientific accomplishment also endangers the integrity of science. Unseemly claims of priority may be encouraged. Premature reports of new scientific discoveries, which will occur to some extent in any circumstances, may be permitted to acquire a semblance of credibility.

An illustration—as yet unrealized—is the matter of "the creation of life." Some scientists believe that the properties of life are inherent in the chemistry of nucleic acid, and would regard the artificial synthesis of a reproducible nucleic acid or nucleoprotein molecule—which may occur in the reasonably near future—as the "creation" of life. Other scientists would disagree with this interpretation because they believe that nucleic acid, nucleoprotein, or anything less than a living cell is not "life," for the reason that it is not a self-sufficient replicative agent.

Under ordinary circumstances this difference of opinion would be occasionally debated among scientists and finally resolved when the weight of evidence on one side or the other became sufficiently strong, or when a new and more acceptable idea emerged. However, in the present circumstances this matter may take another course. There is some evidence that a claimed "creation of life" based on the test-tube synthesis of an infectious molecule might be regarded by a government as a scientific accomplishment of great political importance—a kind of "biological Sputnik." In this case, scientists may be hard pressed to persuade government officials—and perhaps even some of their colleagues—that the discovery should be given an interpretation which is less dramatic but more in keeping with the divided scientific opinion of its significance.

It is evident that the accelerating progress of science has evoked a number of serious problems that affect both the social order and the internal situation of our scientific establishment. Having become a major instrument in political affairs, science is inseparably bound up with many troublesome questions of public policy. That science is valued more for these uses than for its fundamental purpose—the free inquiry into nature—leads to pressures which have begun to threaten the integrity of science itself.

Scientists' Approaches to Their Social Responsibilities

It can be seen from the foregoing discussion that the scientific community is faced with numerous problems that very seriously affect the development of science and the future of society. How have scientists responded to this challenge?

Since World War II there has been a considerable growth in scientists' participation in political affairs. The growth has been intermittent, and based on a variety of views of the scientists' relation to social problems.

The Federation of American Scientists, initiated by scientists involved in the wartime atomic bomb project, is frankly designed to give scientists a direct voice in discussions of political matters that relate to science. The Pugwash movement and the less formal groupings represented by the *Bulletin of the Atomic Scientists* take the view that scientists can serve a useful function in proposing political solutions to the international problems that result from the applications of modern science. This approach results in a deliberate effort, on the part of these scientists, to persuade government agencies to follow a recommended course of action. A third group is typified by the Society for Social Responsibility in Science, which takes the view that scientists have a moral responsibility to try to limit to ethical uses the applications of science and technology.

In the 1956 Presidential campaign, *ad hoc* groups of proponents for both political parties developed among scientists. During the past year both parties have organized scientific advisory committees, which will presumably provide the "scientific authority" for the positions that these parties will take in the 1960 campaign.

Finally, some scientists take the view that their proper role with respect to questions of policy that are related to science is to bring to public attention the relevant facts and scientific principles and an explanation of the limits of accuracy and alternate interpretations that apply. Thus informed, the citizen is prepared to make his own choice between possible solutions. This approach has been the basis for the formation in St. Louis of the Citizens Committee for Nuclear Information, a group of scientists and citizens devoted to the dissemination of information about the radiation problem. A group of scientists with a similar purpose, the Scientists Committee for Radiation Information, has been organized in New York City under the auspices of the New York Academy of Science.

This account shows that scientists are trying to meet their social responsibilities in a variety of ways. It also suggests that no single approach has as yet won the active participation of more than a very small part of the scientific community. Nor has there been a sustained development of these activities. Indeed, in the last few years activity on these matters within the scientific community has been relatively slight.

If we regard participation in the resolution of public issues related to science as a part of the scientists' professional responsibilities, we must conclude that the scientific community has not yet developed a consistent, widely supported way of meeting this obligation.

A Suggested Approach

This committee and its antecedent groups have developed a distinctive approach to the matter of how scientists and their professional organizations (and in particular, the AAAS) can best function with respect to the public issues that involve the progress of science.

To begin with, we suggest that the issues fall into two classes with respect to where, in our social structure, their ultimate solutions lie. Certain problems—for example, the effects of public policy on the development of science itself—are matters in which scientists, as scientists, have a particular interest, responsibility, and experience. In the solution of these problems, the opinions of the scientific community should carry some special weight, and scientists should accept the obligation to develop and explain these opinions.

The more difficult problems are those which do not so exclusively concern scientists but which have a broad relation to public policy and affect all citizens equally. An example of such a problem is public policy in relation to nuclear energy, but this is only the most obvious of a growing class of troublesome issues.

It is our view that such problems are in essence social and political. We expect the choice among alternative solutions of these problems to be made through the normal, democratic processes of social and political decision-making, in which all citizens participate.

In this respect the general issues that relate to scientific developments do not differ from other social and political questions. The difference between them lies at a less fundamental level. In the case of the more familiar questions of public policy, the facts which the citizen, or the government official, requires to make an informed choice between alternative solutions are relatively accessible and the consequences of different solutions are more or less apparent. On the other hand, the factual background and implications of the issues with which we are concerned involve scientific and technical data, often in areas relatively new to science, which are in themselves complex. Many citizens are neither familiar with science generally nor well informed about the specific developments which are at the root of present public issues. Scientists as well as other citizens often lack the relevant scientific facts and are unable to visualize the effects of alternative courses of action. In these circumstances, there is little reason to hope for informed decisions about questions of public policy that relate to science.

In our view, this deficiency is a major cause of the difficulties that now impede the proper development of public policy on science-related issues. This conclusion can be documented in detail from recent experience regarding public policies on radiation hazards, food additives and insecticides, the significance of space exploration, the nature of modern warfare, the population question. This list also illustrates the importance which such issues have assumed in public affairs.

The foregoing analysis leads to a distinctive view of the part which the scientist and his professional organizations should play in the social processes involved in the resolution of science-related issues.

With respect to the process of decision-making, the scientist's role is simply that of an informed citizen. Like any other citizen, the scientist is free to express his opinions regarding alternative solutions for matters of public policy and will perhaps join with like-minded citizens in a group effort to foster the solution he prefers. This role does not derive from the scientist's professional competence or obligations but only from his citizenship, and therefore it bears no direct relationship to his professional organizations.

But in the matter of providing citizens with the knowledge required to make informed decisions on science-

related public issues, the scientist and his organizations have both a unique competence and a special responsibility. As the producer and custodian of scientific knowledge, the scientific community has the obligation to impart such knowledge to the public.

The scientific community has another special competence (which derives naturally from its concern with new and potentially significant attributes of nature), for attempting to detect incipient problems before they become unnecessarily acute. For example, the likelihood that the relation between nutrition and the development of cancer would eventually become a practical problem for the food industry—a matter which is at present agitating farmers and food processors—has been apparent from the work of investigators in many countries for the past 15 to 20 years.

Early detection of such problems is one of the most important direct contributions science can make toward their solution. Too often the most serious obstacle to the solution of such issues is that they are recognized only after the commitment of massive and essentially irreversible economic and social investments. If the Los Angeles area were now about to be settled, it would be a relatively simply matter, given our present knowledge about the causes of smog, to make plans that would prevent a future smog problem. How much more costly is the real situation, which may require that an entire community's reliance on gasoline-powered transport be altered! In its fields of competence, foresight is a capability and, in our view, a responsibility of the scientific community.

It follows, then, that the scientific community should accept the obligation to determine how new advances in our understanding and control of natural forces are likely to affect human welfare, to call these matters to public attention, and to provide for the public and its social and political agencies the objective statements of the facts and of the consequences of alternative policies that are required as the basis for informed decisions on the relative merits of proposed courses of action.

At what point in the social process should the scientific community enter as an agency of information? One view is that, since most social decisions are executed by government, the scientist's function is to inform and advise government departments and officials. The government does, of course, need such advice, and a number of useful methods of providing it have been evolved. In these instances, scientists serve only by invitation. Inevitably, the general content of the information that is provided and the tenor of the advice that is offered are to some degree conditioned by the particular interests of the requesting agency, which determines what questions are asked and who is given an opportunity to answer them.

Such a relationship does not wholly fulfill the scientist's social role, as we see it. In dealing with social issues, the scientific community must demonstrate its responsibility and its inherent regard for truth and objectivity and must zealously preserve the freedom of thought and communication that is essential to the pursuit of these goals. Accordingly, we believe that the scientific community ought to assume, on its own initiative, an *independent* and *active* informative role, whether or not other social agencies see any immediate advantage in hearing what the scientist has to say.

We believe, also, that what scientists have to say about the social implications of science should be addressed directly to the general public. Our traditional preference for democratic procedures requires that the citizen be sufficiently informed to decide for himself what is to be done about the issues that scientific progress has thrust upon us. Furthermore, our command over natural forces—for example, the destructive potential of nuclear war—is now so great as to create social and moral questions of such great moment that no social agency ought to intervene between the issue and the public.

In sum, we conclude that the scientific community should, on its own initiative, assume an obligation to call to public attention those issues of public policy which relate to science, and to provide for the general public the facts and estimates of the effects of alternative policies which the citizen must have if he is to participate intelligently in the solution of these problems. A citizenry thus informed is, we believe, the chief assurance that science will be devoted to the promotion of human welfare.

Questions of Immediate Importance

Many specific problems command the attention of scientists who are concerned with meeting their responsibility to mediate the interaction between science and society. In choosing certain issues for emphasis, we have adopted the view that it is more important to learn how to deal with the difficult problems than with the simpler ones.

The problems presented below have been chosen with this in mind. Other issues may appear to be more important to other observers; we shall welcome our colleagues' recommendations in this regard.

1) *The social consequences of technological progress.* It is characteristic of the present situation that scientific advances lead to a very profound level of control over our environment and to widespread effects on nature. Often the benefits which are the original aim of a particular application of science are accompanied by secondary effects that cause unanticipated harm. The application of new scientific advances calls for social decisions which weigh the benefits against the disadvantages, and the public needs to have the facts relevant to such a decision. The scientific community faces an immediate need for developing the necessary educational programs. Important examples of such problems include: (i) the general effects of technological advances, such as that of automation on industrial development, or of rapid social changes on health; (ii) the effects of radiation from military and peaceful applications of nuclear energy; (iii) the effects of new organic insecticides, food additives, and food colors on animals and man; (iv) artificial control of the weather; and (v) population control.

2) *The association of scientific research and military activities.* Military usefulness is, at present, a dominant motivation in the social support of scientific research and has a profound effect on the development of our scientific establishment. Any significant change in the pattern of military activities—disarmament, for example—is likely to cause serious changes in research opportunities. The close association of science with recent military advances tends to foster a public image of science and the scientist which is not in keeping with the inherent goals of the discipline. The secrecy associated with military applications may restrict the development of science. Some observers regard the problem of preventing the catastrophic application of the power of science in war as a matter which overshadows all others.

There is an obvious need for the scientific community to give attention

to the wide range of problems arising from the close linkage of science and military activity. The role of science in possible efforts toward disarmament and the practical impact of disarmament on scientific research are of immediate concern.

3) *International aspects of science.* Science figures prominently in the intense political rivalry among the major nations of the world. This use of science tends to conflict with its basic international character, and means must be found to resolve this difficulty. A useful innovation has been the development of collaborative international scientific programs, such as those associated with UNESCO, the World Health Organization, CERN, and the IGY. A number of proposals for similar programs in medicine, space research, and oceanography have been made. This area is a fruitful field for developing new ways to foster a sound development of collaborative science. Of particular importance are international programs to provide scientific and technological assistance to underdeveloped nations.

4) *Government support for scientific research.* This problem has received considerable attention in the last few years, but two recent discussions [the Parliament of Science (March 1958) and the Symposium on Basic Research (May 1959)] have shown that no adequate solution is in sight. The basic difficulties seem to be the absence of any over-all rationale in the support of science and the overemphasis on projects that give promise of immediate practical results. We find, as a consequence of this emphasis, that the major part of governmental research support (about 87 percent in the projected 1961 budget) is in the military area, that basic research is inadequately supported, that the pattern of support is not conducive to the development of free inquiry into nature, and that the narrow base of support is distorting the development of science as a whole, in our universities in particular. Various solutions have been proposed, none of which has as yet received really wide support in the scientific community. A specific example is the proposal for the establishment of a federal Department of Science. Clearly, further analysis and discussion of this problem are greatly needed.

5) *How can scientists best meet their social responsibilities?* From what has been said above it is clear that the scientific community has not yet developed a widely accepted means of performing its function in connection with public issues related to science. It would be useful, therefore, to stimulate discussion among scientists on how such activity can best be developed and to encourage efforts which seem to promise success.

6) *The integrity of science.* As science becomes more deeply involved in the frequently discordant affairs of public life and in highly competitive social endeavors, we may expect a growing pressure toward relaxation of the traditional rules for the conduct of science: objective, open communication of results; rigorous distinction between fact and hypothesis; candid recognition of assumptions and sources of error. It is these rules which permit science to progressively increase our understanding of and control over nature. Without them science becomes useless, and even dangerous to the social order. If the scientific community is to accept the obligation to participate in public affairs, means must be found to strengthen the discipline's rules of conduct. Some observers favor the adoption of a code of ethics; others propose less direct means of maintaining scientific objectivity. To begin with, there is a clear need for candid discussion of this problem.

Proposed Procedures for AAAS Consideration

We wish to confirm the general procedural approach suggested by our antecedent committee in its 1958 report to the AAAS Board of Directors. In summary we suggest that AAAS activities follow these steps with respect to any given issue:

1) *Stimulation of discussion, within the scientific community, of issues relating science and human welfare.* Such discussion would result in the identification of issues which the scientific community regards as of most immediate interest and would serve as a guide for the development of a specific program. We urge our colleagues to submit articles and comments for publication, and we welcome direct communications to this committee as well. Symposia and other discussions on these matters should be organized in connection with the AAAS annual meeting.

2) *Assembly of the facts relevant to a given issue.* We propose that a detailed report directed toward the scientific community be prepared after an issue has been identified. Such a report would include the relevant data, a discussion of assumptions and sources of error, and a description of the expected consequences of alternative courses of action. The report would not recommend a specific course of action. The report would be made widely available to the scientific community, either directly or by publication in a suitable journal. For the preparation of such reports we would rely on *ad hoc* committees, conferences, and symposia.

3) *Preparation and dissemination of reports for the general public.* We propose that the content of the report should then be translated into forms suitable for distribution to the public through all available channels. This step is part of the program of the AAAS Committee on Science and the Public and would be carried out in accordance with plans which that committee is developing. An important aspect of these activities is the proposed appointment of a new member of the AAAS staff to supervise work in this area. Our own committee will cooperate in this stage of the program.

4) *Development of liaison between scientists and the public on a local basis.* It is expected that, as the foregoing program progresses, citizens will develop an increasing interest in learning more about the facts relevant to a particular issue directly from scientists in their own community. Many scientists report an increasing demand from local civic groups for lectures on contemporary issues. As already noted, in some communities scientists have formed organizations devoted to informing the public about radiation problems. The British Association for the Advancement of Science has organized local groups to meet public demand for information on these and related problems. The success of these activities suggests possible extension, both geographically and with respect to the types of issues considered. Our committee may serve a useful function in stimulating such developments.

Conclusion

In this report we have reviewed the momentous problems which result from the interactions between the social order and the progress of science. We con-

clude that the scientific community bears a serious responsibility for helping to solve these problems, and have suggested a program that might accomplish this aim.

Such a program does not yet exist, and it would be appropriate for the AAAS to help bring it into being. The task is not an easy one. It will add to the scientist's burden of work; it will require from the citizen more attention to public affairs; it will demand new social inventions. But we believe that a society capable of producing the enormous new powers of science ought to be capable of finding the means of comprehending their effects on the social order. And we are confident that with such understanding, science—as an expression of the creative gifts of the human mind—will flourish, and the power which it endows will be turned more fully to the promotion of human welfare.

References and Notes

1. *Science* **125**, 143 (1957).
2. Premises quoted from the report of the Interim Committee [*Science* **125**, 147 (1957)].

Some Vistas of Astronomical Discovery

New developments in instrumentation have opened up new possibilities in optical astronomy.

W. W. Morgan

As is the case with all epochs, the present one has its own characteristics, both in its political and its scientific aspects. The science of 1960 is not the science of 1920, and, most emphatically, the astronomy of 1960 is not the astronomy of 1920.

The differences are not entirely differences in instrumentation and in techniques; even if the instruments of the 1950's had been identical with those of 1920, we would have been doing different things with them, because our thinking has matured and our approximation to reality in the astronomical field has improved through the investigations of the intervening years.

But the appearance of the new fields of radio astronomy and space science, with the accompanying revolution in instrumentation, is certainly the most spectacular astronomical development of the past 30 years, and the implications for the future in these fields lie beyond the range of the imagination.

However, the field of optical astronomy has also its charms for the future —even for earth-bound astronomers and telescopes, and I should like to describe some of these possibilities as they appear to a simple observer. The omission of further reference to radio astronomy, and to space, does not mean that I consider these fields unimportant but is, rather, a confession of inadequacy on my part to discuss them.

Continuities in 19th-Century Science

A perceptive observer, Jerome S. Bruner, emphasized a basic development in 19th-century science—the recognition of continuities—and he has pointed out the two giant figures in this development, Charles Darwin and Sigmund Freud. The former removed, dispelled, the concept of discrete categories for living forms and showed that organic connections, relationships, exist everywhere, and that man himself could no longer be considered a completely detached phenomenon.

Freud showed that in the case of the mind itself such absolutes as conscious-unconscious and sane-insane have to be abandoned, and that, in the case of these apparent opposites, a continuous sequence of phenomena has to be considered. In addition, developed and archaic qualities exist simultaneously in the same human mind.

Continuities in Astronomy

The development toward a concept of continuities has been a major feature of 20th-century astronomy. The idea of some uniqueness in the location of the earth, or the solar system, received three destroying blows in this century: (i) the demonstration by Shapley that the solar system is located far indeed from the central region of what Kapteyn had called "The Sidereal System"; (ii) the demonstration by Heber Curtis and by Hubble that our own stellar system is by no means unique, and that, on the contrary, multitudes of such systems exist; and (iii) the revision of the scale of distances by Baade, with its accompanying conclusion that our own stellar system is not unique in size. And, as we pass through the mid-century period, we find that more detailed study of astronomical problems brings us inevitably to a broader and broader concept of continuities in this field.

The "either-or" approach grows progressively more inadequate to describe the newly discovered shadings and relationships between phenomena. The concept of "giant" and "dwarf" stars, with its great importance in the historical development of stellar astronomy, has had to be modified successively by the introduction of subdividing categories—subgiants, supergiants, subdwarfs—and, finally, by the recognition of continuities.

The picture that developed out of Baade's brilliant discovery of the two stellar populations has had to be successively modified, first by recognition of an increasing number of subdivisions and now by development of the concept of a continuity of populations lying

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all along the range between Baade's extreme population I and extreme population II.

In the field of star clusters, an equally drastic change in point of view is taking place; the two historical categories of globular clusters and open clusters are becoming indistinct, and in their places comes the realization that the star-clustering phenomenon is probably a continuous one—from the extreme examples of halo globular clusters to the clusterings of stars in dense clouds of hydrogen. There can be no doubt that this progression toward recognition of continuities will develop further in the future.

Some Characteristics of the Present

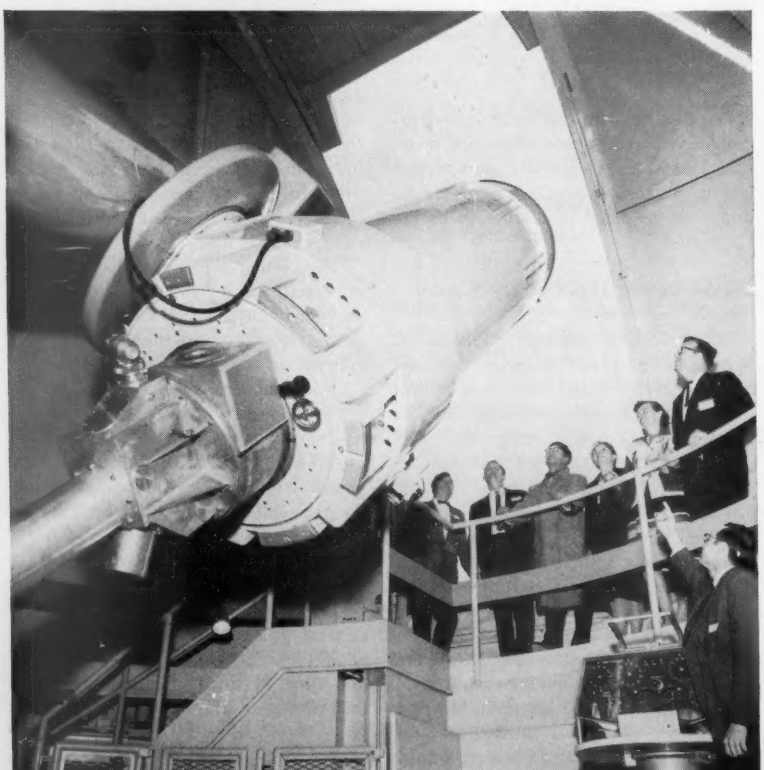
If we limit ourselves to optical astronomy from the earth's surface, certain tendencies seem to be clearly visible. The first of these is the revolution in instrumentation.

At the present time, to my knowledge, there are no firm plans in the Western countries for the construction of optical telescopes larger than the giants at the Palomar and the Lick observatories; but a great improvement in quality and applicability is taking place in the design and construction of telescopes of intermediate size, and (again to the best of my knowledge) the most significant developments in this direction are those going on at the newest of American observatories, Kitt Peak National Observatory.

Overshadowing all other instrumental development is that of image intensification. The real breakthrough in this field has already occurred, and it seems certain that within a very few years the image intensification technique will be in use throughout the astronomical world. This development is, by itself, causing a complete re-examination of the types of telescopes that are most suitable for the problems of the present and future.

The requirement of high aperture for the photographing of faint stars now is being reconsidered, and the advantages of the longer focal lengths in combination with image intensification techniques are becoming clear. One result of this change in emphasis is that certain telescopes now considered to be in the twilight of their careers of prime usefulness may have a recrudescence of life. The 40-inch refractor of the Yerkes Observatory is an interesting example. After a long and honorable career in the forefront of world astronomical research, it has seen the great reflectors take over the commanding role in almost all branches of modern astronomy. For the brighter stars, the great optical stability of the 40-inch has given it a continuing pre-eminence for the precise determination of relative positions of stars; however, this is almost the only field in contemporary astronomical research where the great reflectors have not superseded it in importance. Now, with the imminent introduction of light-amplification methods into astronomy, the speed and efficiency of the 40-inch refractor can be increased to such a degree that it may again have a wide area of usefulness in astronomy.

Of course, if another breakthrough were to take place in increasing the speed of photographic materials, the gain in usefulness of long-focus refractors would also be very great; if photographic emulsions could be increased in speed by a factor of 20, the Yerkes 40-inch telescope would have prime usefulness in the fields of galaxies, star clusters, and other basic areas of research of the mid-20th century.



Visitors to Kitt Peak National Observatory on dedication day inspect the 36-inch telescope.

Vistas of the Future

In site surveys for new observatories, one of the most important factors is the quality of the seeing—or the atmospheric steadiness of the star images. However, there have also been other requirements, such as proximity to roads and other factors affecting living conditions. In the future it may well be that factors other than seeing and transparency of the sky can be minimized in importance in favor of obtaining viewing conditions of absolutely highest quality. The observatory on Pic du Midi in France has seeing of superlative quality; access to this observatory is difficult, and living conditions are satisfactory only because of great preparatory efforts; however, in these days of major technological advances a very great range of sites becomes available, and

comfortable living conditions can be provided almost anywhere.

Because of these developments, surveys can now be carried out that have as their purpose the location of the finest possible observatory sites, with much less attention given to accessibility than in the past. Then the problem of installing the best possible instruments on these sites and arranging for satisfactory living conditions becomes a technological one. It seems quite possible that this approach may furnish sites for future observatories far superior to any now in use on the earth's surface, and intermediate in quality between the best of existing observatory sites and those contemplated for future establishment on vehicles outside the earth's atmosphere.

And now, let us look for a few moments at some of the vistas in astronomical research itself. The safest predictions for the future are those in areas where breakthroughs in method have occurred recently—methods which have not yet been utilized, except in a preliminary fashion. I shall refer briefly to several instances of this kind.

1) A few years ago, Bengt Strömgren developed a method whereby, from precise measurements of a star's radiation within certain narrow, specially selected spectral regions, he was able not only to determine the true luminosity, or candle power, of a star but also to derive the stage in evolution of the star—and therefore, in principle, its age. This method of Strömgren's, as used by himself and by his pupil D. L. Crawford, now makes possible the determination of distances and ages of hundreds of thousands of stars, and this with the use of telescopes no larger than the completed 36-inch reflector of the Kitt Peak National Observatory.

And what problems can be solved by this kind of observation? Problems concerning the precise nature and location of the spiral arms of our galaxy and the arrangement in space of stars of varying ages and physical characteristics, and there is even the possibility —with the giant telescopes of Palomar and Mount Hamilton—of investigating the physical similarities and differences of stars in the galaxies nearest to our own.

2) One of the most active fields in present-day astronomy is the study of galaxies, or great clusterings of stars similar to the Milky Way system in which our sun and planets are located. Investigation by means of the spectroscope shows that some galaxies are composed principally of hot, blue stars and gas, while others are composed principally of cooler yellow and red stars, there being little evidence of large quantities of gas; intermediate between these two categories are galaxies having mixed characteristics, of hot and cooler stars together and intermediate amounts of gas.

Now a relationship exists between the forms of the galaxies and the stellar populations just described. The galaxies that contain the greatest proportion of extremely hot stars and gas turn out to be irregular in shape, with little concentration of their light toward their centers. On the other hand, galaxies composed principally of the cooler yellow-red stars are found to possess extremely luminous central regions and a generally symmetrical appearance. From an extension of these conclusions it becomes possible to make an approximate determination of the stellar and gaseous characteristics of galaxies from a simple inspection of their forms. And so, a method is furnished whereby the average stellar-population characteristics of galaxies can be determined over extended regions of the universe.

I could describe many other instances of exciting possibilities for astronomical research in the future; however, we must content ourselves here with the statement that at no time in the history of modern astronomy have there been so many golden pathways toward the expansion of our basic knowledge of the universe; never has there been such a rich range of possibilities for fruitful investigation by those now working, or by those expecting to enter the field of astronomical research.

Science in the News

Basic Research in the Defense Department: The Department's View

Final agreement on the $40 billion Defense budget was reached last week just before the congressional recess began. The appropriations include over $4 billion for RDT & E—research, development, test, and evaluation. A small proportion, roughly 3 percent, of this will be spent on basic research, but a small proportion of this huge sum makes up a very significant portion of the total amount of money the government spends for basic research.

The official budget lists $136 million, but together with other sums that properly should be considered basic research, the Defense Department will probably spend $150 million during the 1961 fiscal year, which began 1 July. This will be about one quarter of the total federal investment in basic research, and compares with a little over $100 million to be spent by the National Science Foundation, the government agency specifically created to support basic research.

Because Defense has a fairly good idea of where it wants to spend its money, its mechanism for giving grants is quite different from that of the NSF. The foundation picks out what seem to be the most promising proposals submitted. Defense identifies certain areas it wants to support and then uses a sort of talent-scout system to seek out people in the universities and foundations who can do some work in the area. Here are a few samples of the areas singled out by an advisory panel last year: physics—work with high magnetic fields; meteorology—wind fluctuations in the upper atmosphere; mechanics—theory of elasticity; mathematics—theory of numbers; geography—exploration and mapping of the polar regions; chemistry—preparation and properties

of ultra-pure materials; biology—genetics.

Defense officials cite as a particularly successful example the grants to Kusch and Lamb, of Columbia, which led to their discovery of the hyperfine structure of hydrogen. The thinking behind this and many other Defense basic research grants has been this: Defense was interested in microwave physics; it felt that almost any advance in this area might turn out to have important practical applications. Kusch and Lamb were not only working in the forefront of this area but they were recognized as outstandingly capable men, and experience has shown that it is as important to support the most promising men as to support the most promising projects. In this case both the men and the project were promising, and the results were a Nobel prize for Kusch and Lamb and the development of Maser extremely low noise amplifying tubes, with important applications in long-range radar, radio astronomy, missile and satellite instrumentation, and other areas.

Until 1957, there was some tendency for top-level officials to play down the need for basic research. But there now seems to be little support in the Department or in Congress for former Secretary of Defense Charles Wilson's "who cares why the grass grows green" school.

Projects such as those of Kusch and Lamb make it an easily demonstrable fact that the military departments have derived tremendous benefits from the basic research they have sponsored. Transistors, which make possible the miniaturized electronic devices that control the latest weapons, were developed by Bell Laboratories on the basis of studies in solid-state physics sponsored by Defense during the early post-war years. Studies in hydrodynamics sponsored by the Office of Naval Research supplied information which later turned out to be necessary for the development of the Polaris guided missile. This weapon, fired from a submerged submarine, is regarded as possibly the most potent in the Defense Department's arsenal. Klystron-tube technology sponsored at Stanford for use in high-energy electron accelerators for studies of subatomic particles led to the missile warning system and the monitoring of air activity deep inside Russia; the Klystrons are a type of super-powered radio amplifying tubes, and they made possible the high-power radar used in the missile warning and long-distance monitoring systems.

Defense research officials, backed by their committees of outside advisers, say the results produced from investments in basic research are so impressive that Defense ought to be doing a great deal more of this work, perhaps double or more the present level. A report prepared by the Department's Advisory Panel on General Sciences and endorsed by the Defense Science Board suggested that it is unwise to set a level of support in terms of dollars. They said that "support . . . should be determined by the availability of competent scientists rather than the availability of funds." But the research program has actually been lagging considerably behind the recommendation of the advisory panels. The totals have been climbing every year since Sputnik, but the total percentage increase for basic research, about 50 percent, has not matched the percentage increase for over-all R & D.

Getting Support

Defense officials dealing with basic research point to several reasons why support is not as great as they would like it to be. According to their view the problem is partly one of budgeting, partly the over-all concept of Defense research and development. They say it is difficult to get a department operating within budget restrictions to set aside money for things which may turn out to be valuable at some unforeseen time in the future when the department feels a strong and immediate need to spend all the money it has for things it can buy this year. The problem is not peculiar to the Defense Department. The difficulty in getting an adequate program in oceanography [*Science* **131**, 1592 (27 May 1960)] stemmed from the fact that although the field was recognized as important to the nation, it was fragmented among a dozen or so agencies none of which was inclined to give it much consideration in comparison with the other programs the agencies had to pay for within their budgets —programs which had a more direct short-range importance.

The problem is made more difficult, Defense research officials say, by the "systems" concept of R & D. There is nothing wrong with the concept, the officials say; indeed it works very well. It applies, or at least attempts to apply, the idea of the assembly line to R & D; that is, it is hoped that the over-all project will develop in such a way that each component of the system will be ready when it is needed. This works well enough for applied research, where it is at least comparatively easy to estimate how long it will take to get something done, but it leaves little room for basic research. It is almost impossible to guess when a basic breakthrough will take place, or even to guess what the exact nature of the breakthrough will be or where the information developed can be put to practical use. This means that, for the most part, the big development projects are based on fundamental research that has already been done. It leads to a tendency to merely assume the existence of basic information and to forget that much of it would not exist except for basic research sponsored in earlier years.

But despite such difficulties there are certain advantages in having a great deal of basic research sponsored by operating agencies like Defense rather than in placing it all under a single outside agency such as the National Science Foundation or the often proposed Department of Science.

Defense officials argue that people who are close to the applied programs necessarily develop an awareness of the areas where research is going to be most valuable; they point out that no change in organization could relieve the Defense Department of the responsibility for seeing to it that basic research is pushed in areas that are vital to national security. To assign all basic research to an independent agency would merely separate assigned authority from inescapable responsibility. It would add another layer of organization to a government which already appears to be so big that it has a sometimes embarrassing difficulty in coordinating its activities and making decisions.

The Defense officials claim that even the occasional criticism that money is wasted on projects that are not very promising is really a further argument for keeping the research within the Department, where it will be close to people involved in applied R & D. They say that it is and should be the policy of the department to occasionally gamble on projects that may appear risky, but which may lead to a major breakthrough. The Defense Department spends billions every year on R & D; it is thus a matter of common sense that it should be willing to gamble a few hundred thousand dollars on a research project that may develop infor-

mation that could save hundreds of millions later on. Officials view it as part of the function of the Defense basic research program to support these risky projects which an agency like the NSF, not so closely aware of potential practical applications, is likely to be unwilling to support. There seems to be substantially less of the "that's been tried before and it isn't going to work" attitude in Defense than in NSF, and the Defense officials argue that that is precisely the way it should be.

Congressional Recess: Some Matters Left Undecided until August

There has been a great deal of speculation over the exact reason for the Congressional recess and even more over what the postconvention session will accomplish. The only precedent is the 1948 session Harry Truman called in order to dramatize his charges about the "do-nothing" 80th Congress. That session accomplished nothing of significance, since the Republicans who controlled it understandably had no intention of carrying out Truman's program. This time the situation is very different: the session is being called on the initiative of the party that controls the Congress, and the Democrats will necessarily be making a determined effort to make at least a start toward carrying out the program of their newly adopted platform.

During the closing hours before the recess, both houses adopted the conference report on Independent Agency appropriations (*Science*, 1 July 1960). This included $175.8 million for the National Science Foundation and $915 million for the Space Agency. Both houses also cleared the "Health for Peace" proposal. Action was put off until August on the HEW appropriation with its big proposed increase in funds for medical research, and on the Antarctic treaty, which in effect internationalizes the Antarctic, outlawing the establishment of military bases there and opening the entire continent to the scientific expeditions of all nations.

One thing that is certain is that federal support of scientific research and development will reach a new high in the fiscal year that began 1 July. It will run close to $9 billion, substantially more than the total of all nonfederal support for R & D and triple the presputnik level prevailing before fiscal 1958—H.M.

News Notes

Eugenics in New Guinea

A plan to introduce a eugenic policy, apparently without precedent anywhere, has just been announced by the administration of the Territory of New Guinea [*South Pacific Post* (24 May 1960)]. This measure affects the tribe of Fore, some 30,000 strong, which inhabits an area of about 884 square miles in the Eastern Highlands. Almost half of the women and one-tenth of the men of the Fore tribe die of a hereditary disease known as kuru. This is a grave neuropathological disturbance for which no cure is known and which leads to death, usually within a few months from the appearance of the first symptoms.

As shown by studies conducted in the Fore area by a group of investigators from the University of Adelaide (South Australia), kuru is transmitted by a gene which behaves apparently as a Mendelian dominant in females and a recessive in males. The presumed homozygotes of both sexes die in childhood, usually before adolescence; heterozygous females die later, after most of them have produced children. The extremely high incidence of kuru in the Fore tribe and its absence in neighboring tribes except as introduced by migrants from the Fore area remain unexplained. The heterozygous carriers of the gene for kuru probably possesss some considerable adaptive advantage, both the males who survive and perhaps also the females who die after having completed at least a part of their reproductive lives. The nature of this advantage is, however, completely unknown.

In part because of the ravages of kuru, some portions of the Fore tribe have great excesses of males over females (as much as 2.5 males per female). Fore men tend to move into neighboring tribes, and some of them, like members of tribes in which kuru is unknown, come as contract laborers to work on plantations in other parts of New Guinea. Since many of them are heterozygous carriers of the gene for kuru, it is feared that the disease may spread and afflict the populations of other districts.

To counteract this danger, the administration's plan is to quarantine the Fore tribe, by prohibiting the emigration of its members from the tribal area. All Fore men who have been recruited as laborers from the area will be returned there. This is obviously a severe restriction to be imposed on a whole tribe. Realizing this, the administration is considering ways and means of developing the Fore area to provide work for its people and to make emigration unnecessary. The study of the disease is to be continued, in the hope of finding a remedy for kuru victims and a way of identifying the carriers of the kuru gene in heterozygous condition before the appearance of the disease symptoms.

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Documentation Center Opened

A Scandinavian Documentation Center (SCANDOC) was opened recently in Washington, D.C., to further the exchange of scientific and technical information and documentation among the Scandinavian countries, the United States, and Canada. SCANDOC, a nonprofit organization offering free service, is financed and directed by the research councils and scientific academies of the four Scandinavian countries through their common Scandinavian Council for Applied Research.

The center will procure nonclassified and nonconfidential documents and information not readily available and will channel this information to the interested countries through information offices organized under the research councils and academies of the countries concerned. Arne Sverdrup, former head of the Laboratory for Steroid Research of the Norsk Hydro's Institute for Cancer Research, Oslo, will head SCANDOC. Sverdrup is also science attaché at the Norwegian Embassy.

Dedication of NIH Building

The Division of Biologics Standards Building of the National Institutes of Health, Bethesda, Md., was dedicated on 30 June. Roderick Murray, director of the division, delivered the principal address, which commemorated the fifth anniversary of the creation of the division as a separate unit of NIH.

The $3.5 million structure was designed to house the scientific and ad-

ministrative operations of the division, which is responsible for the safety, purity, and potency of all biological products within the jurisdiction of the Public Health Service.

King Bhumibol Adulyadej of Thailand was invited to dedicate the new building in recognition of his work to improve health measures in his country and his interest in the SEATO–NIH Cholera Research Project. The work of U.S. and Thai scientists during the 1958–59 cholera epidemic in Thailand demonstrated, by a greatly reduced death rate, the value of collaborative research. The U.S. cholera research advisory group was headed by Joseph E. Smadel, NIH associate director for intramural research, who will become chief of the laboratory of virology and rickettsiology of the Division of Biologics Standards.

Symposium on Experimentation below the Microgram Range

The National Academy of Sciences–National Research Council recently sponsored a symposium on Experimentation below the Microgram Range. Attendance was limited to 27 invited participants. The proceedings will be published in a separate issue of the *Microchemical Journal* this fall.

Nicholas D. Cheronis of Brooklyn College was chairman of the planning committee and general chairman of the symposium. In his opening address, Cheronis stated that two of the principal objectives of the meeting were (i) to stimulate interest in the field and (ii) to gather investigators from separate areas of the natural sciences to exchange information on the problems of such experimentation.

Henry Eyring, dean of the Graduate School of the University of Utah, delivered the closing lecture, in which he discussed the theoretical aspects of kinetics of chemical reactions in the microgram and submicrogram range. He asserted that surface reactions are more critical at the submicrogram range.

Committee Established for Cell Culture Collection

The Cell Culture Collection Coordinating Committee was established on 3 June on the recommendation of the advisory panel on viruses and cancer of the National Advisory Cancer Council.

The committee's primary aim is the development of criteria to certify strains of mammalian cells. Meetings between virologists and cell culturists, which were sponsored by the National Cancer Institute during the past year, indicated the need for a new disciplinary approach to the study of cell cultures.

Fourteen U.S. laboratories will assist the committee; two will undertake most of the cell line characterization and one will act as a central repository and distribution center. Lewis L. Coriell, research director of the South Jersey Medical Research Foundation, Camden, and Cyril S. Stulberg, senior research associate in the Child Research Center of Michigan, Detroit, will establish the first centers for the study and banking of cell strains. William A. Clark, director of the American Type Culture Collection, Washington, D.C., will direct the central repository.

In addition to Coriell, Stulberg, and Clark, the committee is composed of the following scientists.

Joseph W. Beard, professor of experimental surgery and associate professor of virology, Duke University School of Medicine.

John F. Enders, chief of the research division on infectious diseases, Children's Medical Center, Boston.

Morgan Harris, professor of zoology and chairman of the department, University of California, Berkeley.

John G. Kidd, professor of pathology, Cornell University Medical School.

Jerome T. Syverton, professor of bacteriology and head of the department, University of Minnesota Medical School, Minneapolis, *chairman*.

All inquiries should be sent to the committee chairman in Minneapolis.

News Briefs

Translating Soviet journals. The Soviet publishing firm Mezhdunarodnaya Kriga and Consultants Bureau Enterprises of New York renewed, for a 2-year term, a contract covering the exclusive translation into English of 23 major Soviet scientific journals. The agreement was concluded between Aleksandr Belostotsky, an official of the Moscow firm, and Earl Coleman, president of Consultants Bureau. The original contract, made in 1958, was the first such agreement between an American publisher and the Soviet Union; the contract was renewed for 1 year in 1959. A special clause in the new agreement requires the Moscow company to notify Consultants Bureau of publication of any new scientific and technical Russian journals.

* * *

Conference on aging. The 5th congress of the International Association of Gerontology will meet in San Francisco, 7–12 August. All papers will present results of original research or systematic evaluations of operating programs. They will cover the biological, clinical, psychological, and sociological aspects of the aging process. Additional information may be obtained from Louis Kuplan, President, Fifth International Congress of Gerontology, P.O. Box 2103, Sacramento 10, Calif.

* * *

Nutritionists meet. A symposium on world food needs and food resources will highlight the program of the 5th international Congress on Nutrition in Washington, D.C., 1–7 September. The balance of the program will be divided into panel discussions and sessions of short papers on unpublished original research. The organizing committee will offer lectureship appointments, through the cooperation of American universities and research centers, and travel grants to aid foreign scientists who plan to attend the congress. For full information write to Milton O. Lee, General Secretary, 9650 Wisconsin Avenue, NW, Washington 14.

* * *

Centennial conference. A conference will be held in London, 20–23 September, to commemorate the centennial of the passage of the world's first pure food act. Papers reviewing the contributions of legislation and technology to the establishment of high standards of food purity will be presented by scientists from Canada, England, the United States, and Wales. Inquiries should be sent to A. J. Amos, Chairman of the Executive Committee, Pure Food Centenary 1960, The Secretariat, 14 Belgrave Square, London S.W.1.

* * *

Teratology Society. Scientists interested in the etiology and morphogenesis of congenital malformations have formed the Teratology Society of America as a result of action taken at the 4th teratology conference, held earlier this year in New York. The National Foundation assisted in the formation of the society. Inquiries should be addressed to the secretary-treasurer, Marjorie M. Nelson, Department of Anatomy, School of Medicine, University of California, San Francisco 22.

Scientists in the News

The Gravity Research Foundation of New Boston, N.H., has presented awards to five scientists for essays.

Lloyd Motz, of Rutherfurd Observatory, Columbia University, received $1000 for his essay on gravity and the nature of fundamental particles.

Banesh Hoffmann, of the department of mathematics, Queens College, Flushing, N.Y., was awarded $300 for his paper on the noon-midnight red shift.

F. J. Belinfante, of the department of physics, Purdue University, received $200 for his essay on whether fast motion or fast rotation or vibration of an object can decrease the effect of gravity on it.

W. F. G. Swann, director emeritus of the Bartol Research Foundation, Swarthmore, Pa., was awarded $150 for his paper on the possibility of developing a shield against gravitation.

Charles J. Lyon, of the department of botany, Dartmouth College, received $100 for his essay on the dependence of plant form and function on gravity.

Charles W. Hargens, head of the bioelectronics branch of the Franklin Institute Laboratories, has been promoted to technical director in charge of electrical engineering. Hargens will direct research and development programs in electronics, life sciences instrumentation, electromechanics, and power and control systems.

Waclaw Szybalski, former head of the microbial genetics unit of the Institute of Microbiology of Rutgers University, is continuing his work on the molecular genetics of bacterial and human cell lines as associate professor of oncology in the McArdle Memorial Laboratory of the University of Wisconsin Medical School.

Harris Busch, professor of pharmacology at the University of Illinois, will become chairman of the department of pharmacology at Baylor University on 1 September.

Arthur W. Galston became chairman of the department of botany of Yale University on 1 July. He will spend the academic year 1960–61 on sabbatical leave in Canberra, Australia. **Norman H. Giles** will serve as acting chairman in Galston's absence.

Ian M. Sussex, of the department of biology of the University of Pittsburgh, became an associate professor in Yale's botany department on 1 July. **Diter H. von Wettstein**, of the Forest Genetics Institute in Stockholm, was also named associate professor in botany.

Donald B. Shutt, dairy bacteriologist in the department of microbiology of Ontario Agricultural College, Guelph, Canada, retired on 30 June. He is a specialist in the flavor and color defects in dairy products, and in dairy sanitation.

Four British scientists are visiting the United States this summer.

H. Davson, a member of the Medical Research Council's scientific staff at the department of physiology of the University College (London), began a 3-month assignment at Woods Hole Marine Biological Laboratory at the end of May.

H. A. Gebbie, principal scientific officer in the basic physics division of the National Physical Laboratory (Teddington), is spending a month at Brigham Young University, Provo, Utah, before attending the Gordon Research Conference on infrared spectroscopy, 22–26 August.

E. King, a member of the Medical Research Council's scientific staff in the department for research in industrial medicine at the London Hospital, will be in New York from 24 to 31 July to attend the International Congress on Occupational Health.

E. G. Youngs, senior scientific officer at the Agricultural Research Council's unit of soil physics (Cambridge), will begin an 8-month visit on 11 August. He will visit the University of Wisconsin, the University of Illinois, and the U.S. Salinity Laboratory (Riverside, Calif.).

Gladstone B. Heisig has been named professor emeritus of chemistry at the University of Minnesota, after 41 years of service.

Ivor Griffith, president of the Philadelphia College of Pharmacy and Science, has been named the 37th recipient of the Remington Honor Medal by the American Pharmaceutical Association. The medal will be presented at the awards dinner on 7 December in New York.

Henry Turkel of Detroit, Mich., is a visiting professor on the Faculty of Medicine and Pharmacy, Port au Prince, Haiti.

E. R. Tompkins is now assistant to the Naval attaché in the U.S. Office of Naval Research in London, where he is responsible for liaison in physical and nuclear chemistry between ONR and European research installations. Tompkins formerly headed the chemical technology division of the U.S. Naval Radiological Defense Laboratory in San Francisco.

Robert J. Anderson, chief of the Communicable Disease Center of the Public Health Service in Atlanta, is now deputy chief of the service's Bureau of State Services.

Harla Ray Eggleston, professor of biology and chairman of the department at Marietta College, Marietta, Ohio, retired on 6 June after 45 years as head of the department. Eggleston is an authority on the ecology of the bivalve mollusca. He received the bachelor's degree from Hamilton College in 1912, the master's from Harvard in 1913, and a doctor of science degree from Hamilton in 1950.

Recent Deaths

Joseph K. Folsom, Boston, Mass.; 66; professor of sociology at Vassar College, 1931–59; former editor of the *American Sociological Review*; 3 June.

Ivan M. Johnston, Cambridge, Mass.; 62; associate professor of botany at Harvard University; associate director of the Arnold Arboretum, 1948–53; authority on American flowering plants; 1 June.

Theodore K. Just, Chicago, Ill.; 55; chief curator of botany of the Chicago Natural History Museum; professorial lecturer in the department of botany of the University of Chicago; 14 June.

William St. Lawrence, Southampton, N.Y.; 72; founder of the clinic for study of heart disease in children at St. Luke's Hospital, New York; associate in the diseases of children at Columbia University College of Physicians and Surgeons, 1920–29; 3 June.

Wladimir S. Woytinsky, Washington, D.C.; 74; former research director of the Twentieth Century Fund and research professor at Johns Hopkins University; economist, statistician, and writer; 11 June.

Constantin P. Yaglou, Belmont, Mass.; 63; professor of industrial hygiene at the Harvard School of Public Health; expert on human adaptation to climatic extremes; 3 June.

Book Reviews

The Politics of National Party Conventions. Paul T. David, Ralph M. Goldman, and Richard C. Bain. Brookings Institution, Washington, D.C., 1960. 592 pp. $10.

It is not surprising that many people are puzzled regarding what to think of our national party conventions. On the television screen we witness a scene of zany confusion. From the political gossips we learn that behind this confusion powerful bosses make deals in smoke-filled rooms. Perhaps we also remember being taught that these gatherings are the highest representative assemblies of our two great parties, where the voice of the people somehow prevails, choosing the best candidates available for our highest office.

If we are puzzled, one excuse is that little systematic study has been made of the nominating process. That excuse is removed by this massive study. (Incidentally, there is also available a paperback edition, condensed to about half the length of the original work.)

The focus of the book is on the conventions as a means of selecting nominees for President. But the authors are also concerned with the broad historical development of American politics and with how it has affected and been affected by the changing process of nomination. Two trends are given particular attention. One is the "nationalizing" of American politics—or to put it negatively, the decline of sectionalism and localism. The great era of sectional politics ran from the first election of McKinley in 1896 to the Al Smith–Herbert Hoover campaign in 1928. Opposed to the solidly Democratic South was a block of states, which, especially in the Northeast, were almost as solidly Republican: in the election of 1896, for instance, the Republicans carried every *county* in the six New England States. This was the heyday of powerful state bosses—Platt, Quay, Hanna—who provided "the main centers of organization and maneuver within the contesting factions" at the conventions. Beginning in 1928 a massive realignment of the party electorate took place, the Republicans becoming more clearly identified with the white collar, professional, and well-to-do classes, and the Democrats with organized labor, ethnic and religious minorities, and the poor.

This shift in the social bases of the parties has been reflected in the background of convention delegates. Far fewer Catholics and Jews take part in the Republican convention. In 1952 the labor caucus at the Democratic convention included about 100 votes, while the labor vote at the Republican convention was probably not over 10. Outstanding businessmen can still be found among Democratic delegates, but they are about twice as numerous at Republican conventions, which also include a much higher number of the very rich.

This electoral realignment and the rise of new economic and social issues also had consequences for the power structure of the parties and their conventions. The decline of the state boss was hastened, as power moved toward national leaders better able to organize and compete on a nation-wide scale. Moreover, this national leadership has become less dispersed and more closely knit. Unlike most political parties in other countries, American parties have often done without any single continuing "inner circle" of leaders. But today the American party, at least when in power, has developed a fairly tightly organized presidential wing. And even in the out party, the position of titular leader has been strengthened. A shadow, however, is still cast on his position by the fact, except for Cleveland, no man who has been once defeated as his party's candidate has been able to go on from renomination to victory in the ensuing election.

Along with the nationalizing of American politics has gone a second major development, an increasing public participation in the nominating process. This has been brought about, in the first place, through the presidential primary, in which party members directly elect delegates or give them some sort of instruction concerning their presidential preference. In 1956 half the delegates to the conventions were elected or instructed in this way. Yet during the interwar years, presidential aspirants did not attach much importance to the primaries, and the hectic preconvention campaign based on a wide use of primaries is the product of very recent times and especially of two men, Harold Stassen in 1948 and Estes Kefauver in 1956.

Public opinion polls, widely reported in the press and over the air, have also enhanced public influence, identifying the front runners and concentrating attention on them. At the same time, close reporting of the sentiments of political leaders and potential delegates, by suggesting which aspirants have a real chance of nomination, tends to eliminate the hopeless cases. One result of these changes has been to narrow the field of aspirants to those who have established their positions in the preconvention campaign, which has come to perform much of the selection process before the convention meets. The position of front-runner has become increasingly desirable; conversely, the chance of a "dark horse" coming forward in a stalemated convention has become less likely.

In the states, the direct primary for state-wide office has tended to weaken party leadership. Yet in the national parties, the integration of top leadership does not seem to have been impeded by a limited use of the primary. In both conventions, however, the old conflict between the congressional and the presidential wings of the party still flares up often. Conceivably, the greater prominence of senators and congressmen as presidential aspirants and convention leaders may in time throw some bridges across this gap.

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Chemical Analysis of Air Pollutants. Morris B. Jacobs. Interscience, New York, 1960. 430 pp. Illus. $13.50.

Public awareness of the extent and hazard of air pollution has grown considerably during the past few years, and as a direct result many industrial organizations and government agencies have been faced with the problem of providing adequate analytical service for measuring the degree of pollution

and controlling it. The nature of air pollution chemistry requires an almost unique analytical approach that differs in both concept and execution from classical techniques. As a result, many techniques have been evolved, but this latest contribution by Morris Jacobs is the first single, comprehensive volume treating only the analytical aspects of air pollution chemistry.

The range of subjects discussed is quite extensive and should cover almost any conceivable situation likely to be encountered by an industrial or municipal air pollution control officer. The detailed treatment of each subject will allow those unfamiliar with chemical analysis to apply the individual methods. The book will be of value to chemists already familiar with the field, but to them the details may seem overpowering at times. This excessive detail is relieved at frequent intervals by discussions of other aspects of the problem such as sampling, data treatment, and physical laws. In addition, there is sufficient theoretical material to allow an easy understanding of the methods involved. This combination of theory and detail is the book's greatest value and will be the feature with the widest appeal. Unfortunately the air pollution problem has grown faster than our inclination to measure and control it. The methods for measurement and control are available, but with the exception of a few of the more enlightened or harassed communities, there has been little application of this knowledge to the actual problem. Most control is still supervised by agencies which have had little experience except in smoke inspection. These agencies are often understaffed with personnel untrained in the pertinent fields; a single volume such as this one can be an invaluable reference source for use in planning and executing control programs.

The materials covered are, with one exception, the usual types of pollutants produced in urban areas. The exception is an excellent chapter on radiochemical determinations. Radioactive air contaminants are a disputed component of the atmosphere, but there is little chance that the increased use of nuclear processes will decrease the amounts present in the atmosphere. It is reassuring to find that this contamination is now considered by an authority such as Jacobs to be as significant as soot, dust, and noxious vapors. To discuss analytical radiochemistry in a single chapter without some omissions is impossible, but his treatment is a more than adequate summary of most of the present techniques.

One chapter detracts from the book's excellent coverage; compared with the other material, the treatment afforded the analysis of the exhaust gas from automobiles seems out of place. Without doubt the exhaust from motor vehicles is an extremely important source of pollution, and for this reason great emphasis should be placed on the detection and determination of this substance or its reaction products as they appear in the atmosphere. Unfortunately, Jacobs emphasizes the analysis of the gases *before* emission into the atmosphere. Unless some radical change occurs in the internal combustion engine, there is little need for pursuing this aspect of the problem.

In general, the book is well arranged. The development of the material proceeds logically from an initial chapter concerning sampling to a final, brief chapter that gives practical discussion of monitoring instruments. Jacobs is perhaps quite familiar with the budgetary problems of most air pollution control agencies and has kindly omitted mention of most of the more exotic and expensive instrumental techniques.

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Medical Physics. vol. 3. Otto Glasser, Ed. Year Book Publishers, Chicago, Ill., 1960 lx + 754 pp. Illus. $25.

Volumes 1 and 2 of *Medical Physics* have well established their value as outstanding standard works in the fields of medicophysics and biophysics. Volume 3, with contributions by 181 experts and pioneers in the various fields, carries on this reputation in an impressive way. Volume 3 does not purely augment earlier topics; it presents recent developments and progress, but it also covers many new subjects which have become of importance and interest in recent years: aviation medicine, the biological hazards of space radiation, fluorescence microscopy, fluorescopic image amplification, grid therapy and grid protection, the ultraviolet color-translating microscope, and modern microradiography, to mention a few.

The presentation throughout makes clear, stimulating reading. The references are well selected, the volume is arranged alphabetically by title of the topics, and a compendious name index provides a quick reference index for the three volumes. It is impossible to review in detail the 177 chapters of the volume, and it is not possible to select the one or the other contribution for detailed comment since every chapter holds its *niveau*. Otto Glasser has to be congratulated for being able to assure the collaboration of so many recognized experts in the different fields. There is no doubt that the new volume will be a welcome addition to the existing medicophysical literature.

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Encyclopaedia Zoologica Illustrated in Colours. vol. 4, *Arthropoda to Protozoa Exclusive of Insecta, Echinodermata, and Mollusca*. Y. K. Okada *et al.* Hokuryu-kan Publishing Co., Tokyo, Japan, 1960. 308 pp. $25.

This handsome volume, although written entirely in the Japanese language except for the scientific names of the animals, will be of widespread interest to naturalists because it portrays in color a significant representation of the Japanese species of the animal groups covered: the Arthropoda (exclusive of insects), Annelida, Nemathelminthes, Trochelminthes, Nemertini, Platyhelminthes, Ctenaria, Coelenterata, Porifera, Mesozoa, and Protozoa. The first 45 pages, devoted to brief accounts of the morphology and classification of the phyla included, are well illustrated with black-and-white diagrams. This section is followed by 123 plates of representative Japanese species of each phylum; mostly in color, and including marine, fresh-water, terrestrial, and parasitic examples.

The color reproduction is excellent on the whole and makes the American reader wish that a comparable guide to our invertebrate fauna were available. Especially noteworthy are the illustrations of arachnids and crustaceans. Also of interest are the colored plates showing some of the more remarkable members of the Japanese fauna: the platyctenid ctenophores; the peculiar genera of stauromedusans; the giant hydroid, *Branchiocerianthus imperator*; the ascothoracican cirripedes; the myzostomids; and the garishly colored polyclads and nemertines.

This volume will be of value to professional zoologists who wish to become acquainted with Japanese invertebrates

as they appear in life and to teachers and students who will have an opportunity to feast their eyes on the remarkable diversity of color and form presented by almost every animal group.

An index to the scientific names of all the species of animals illustrated provides a useful guide to the book for Western readers.

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Digital Computing Systems. Samuel B. Williams. McGraw-Hill, New York, 1959. x + 229 pp. Illus. $7.75.

An Introduction to Electronic Data Processing. Roger Nett and Stanley A. Helzler. Free Press, Glencoe, Ill., 1959. 287 pp. Illus. $6.75.

Digital Computer Primer. Edward M. McCormick. McGraw-Hill, New York, 1959. x + 214 pp. Illus. $7.50.

A Primer of Programming for Digital Computers. Marshal H. Wrubel. McGraw-Hill, New York, 1959. xv + 230 pp. Illus. $7.50.

Programming for Digital Computers. Joachim Jeenel. McGraw-Hill, New York, 1959. viii + 517 pp. Illus. $12.

Programming Business Computers. Daniel D. McCracken, Harold Weiss, and Tsai-Hwa Lee. Wiley, New York; Chapman and Hall, London, 1959. xvii + 510 pp. Illus. $10.25.

Up until several years ago, the published literature on electronic digital computers was very scanty. Most of the information was available only to those active in the field and was in the form of internal reports and memoranda or was passed on as part of an oral tradition. With the rapid spread of the use of computers throughout the economy, it became necessary to make this material accessible to the various groups whose interests were affected by the rapid development of these data processing systems. Many books on computers have appeared within the past few years, and the present group provides a relatively good sample of the types of books on the market.

Digital Computing Systems is concerned primarily with the hardware of computers and is written for those familiar with electrical circuits and apparatus. The five major components of a computing system—input, output, storage, arithmetic, and control—are dealt with in the major chapters. Peripheral subjects such as historical background, number systems, programming, and applications are also discussed. There are many illustrations of computer elements and numerous circuit diagrams. However, the frequent mention of relays and other components which are on their way out gives the book an out-of-date flavor. In addition, it is poorly motivated and has no apparent over-all design. It may have some historical interest, but one who is interested in the present state of computer equipment should look elsewhere.

An Introduction to Electronic Data Processing also describes the five major elements of an information-processing system, but from a functional viewpoint. It emphasizes what a computer can do, not how it does it; it is intended for the administrator considering the advisability of installing a computing system in his concern and tells what is involved in the operation of such a system. The discussion covers the problems of personnel as well as the difficulties resulting from conversion to the computer-oriented system. This conversion problem, in all its ramifications, has been a costly stumbling block for many organizations which have been sold a computer as the solution to their growing paperwork problem but which have not been alerted by the salesmen to the revolutionary impact this would have on their entire operation. The book concludes with an appendix describing electronic data processing systems of several leading manufacturers.

Digital Computer Primer is written for the well-informed layman. As such, a rather complete picture of the computer business is given in elementary terms, but applications are not discussed. There are some inaccurate generalizations which the author admits making to simplify the picture; he hopes that, with time, the exceptions will disappear as a greater degree of standardization is attained among computer manufacturers. Other causes for complaint are the stress given to I.B.M. equipment and the lack of clarity in the explanations of circuits. However, these are minor criticisms of a good introductory text on general-purpose, stored-program automatic digital computers.

One of the principal bottlenecks in the operation of a computer center is programming, which may be broadly defined as the process of taking a problem presented by a potential machine user, reformulating and analyzing the problem from a computer's point of view, preparing a set of instructions for the computer to solve the problem, checking that these instructions as written do what the programmer intends them to do, and finally making sure that proper safeguards are taken against all kinds of mishaps that may occur during the actual running of the problem. Programming a complex scientific, logical, or commercial problem may take several man-years, and with the abundance of computers and problems, there is a shortage of qualified programmers. This shortage is being met, on the one hand, by formal courses in programming taught in the colleges, and on the other hand, by automatic programming systems using the computer itself to perform many of the routine tasks of programming.

A Primer of Programming for Digital Computers is meant to be a text for an introductory course in programming for scientists and engineers. The title is slightly misleading since it is exclusively concerned with programming of the I.B.M. type 650 computer. However, this is justifiable to a certain extent, for you can program any computer with little additional effort once you have learned how to program one machine well. Furthermore, since 650's are available in many universities and since a potential programmer can only realize the pitfalls in programming by actually testing several of his programs on a machine, a text on programming for the 650 is very practical. The book is divided into two parts, elementary programming and advanced programming, but even the second part is elementary and only scratches the surface of the subject.

Programming for Digital Computers is a much more thorough treatment of the discipline of programming, which deals with the techniques in an almost abstract manner. This does not preclude its being a practical book; in fact, much valuable information of a practical nature is found in this admirable presentation of the subject. This book is a must for anyone who wishes to become a professional programmer.

Programming Business Computers is intended for these members of a business organization, who will be directly concerned with programming the organization's problems on a data processor, as well as for their supervisors. The consensus among experts in commercial computer work is that it is more efficient to teach programming to one familiar with the workings of

a firm than to teach the methods of a long established business to a professional programmer. This book covers almost everything about programming and computers, needed by such personnel, except, of course, the specifications of the particular system installed in their office. As in a previous book on programming by McCracken, a hypothetical computer, DATAC, incorporating features of the various business computers on the market, is described and used in the text rather than any existing computer. This is a disadvantage; as I indicated above, one learning to program for a computer should run several programs on that computer. However, there is a good chance that, in view of the excellence of this book, DATAC simulators will be written for various existing computers. This will enable one to run problems written in the DATAC code on a particular computer.

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Ascidiacea. *Discovery Reports*, vol. 30. R. H. Millar. Cambridge University Press, London, 1960. 160 pp. Illus. 70s.

This monograph is based on 2500 specimens representing 78 identifiable species, principally from the Falkland Islands and the Patagonian shelf, but also from other subantarctic islands and some material from New Zealand as well. There are 13 new species.

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Science Study Series. *Horns, Strings, and Harmony*. Arthur H. Benade. 263 pp. Illus. $0.95. *The Restless Atom*. Alfred Romer. 192 pp. Illus. $0.95. Doubleday, Garden City, N.Y., 1960. (Available to secondary school students and teachers from Wesleyan University Press, Columbus, Ohio.)

These most recent volumes of the "Science Study Series" maintain the high standards set in its previous books. The series is being published by the Physical Science Study Committee as one part of its program for improving high school physics education. The books are intended to be read profitably by young students and by laymen. Amazingly enough, they seem to be successful on both levels.

The new books are well written. This does not necessarily mean that they are easy; each book will demand careful, hard reading from the nonprofessional reader. However, if he is sufficiently interested in the subject or if his intellectual curiosity is great enough, his work will be well rewarded.

Horns, Strings, and Harmony was written by Arthur H. Benade (Case Institute of Technology). Benade's main field of research is in nuclear physics, but he continues an old Case tradition by studying the physics of music.

Following an introduction that is partly autobiographical, Benade presents some of the physics necessary for understanding musical vibrators. He then discusses how the structure of the human ear determines our hearing of music. The various types of musical instruments examined range from strings to brasses to woodwinds. The final chapter applies the information found in the earlier sections by giving instructions for constructing simple trumpets and woodwinds.

Benade's book communicates well his love for both music and physics. Not only is the reader reminded of the compatibility of music and physics, but he sees how the study of either subject can provide some valuable understanding of the other.

Alfred Romer (St. Lawrence University) wrote *The Restless Atom*. It is a clearly written, exciting account of the revolution which occurred in physics between the 1890's and the 1910's.

Romer does not attempt to give a complete history of atomic physics; he stops after indicating the first successes of Rutherford and Bohr. Relativity and quantum mechanics are hardly mentioned. Nevertheless, Romer gives the reader a good appreciation for modern understanding of the atom. He accomplishes this by telling his limited story well.

He starts his story with a brief description of Röntgen's discovery of x-rays, backtracks to describe the understanding of the elements current before Röntgen's work, and then proceeds to describe the rapid developments which followed. Romer recounts the work of Becquerel, the Curies, the young Rutherford, Crookes, Soddy, and Bohr, among others. He presents the problems that confused them, their groping for answers, and the gradual creation of a workable picture of the atom.

The Restless Atom leaves the reader eager to learn more about the development of quantum mechanics, and it gives him a good background for such study.

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Moisture in Textiles. J. W. S. Hearle and R. H. Peters, Eds. Textile Book Publishers (Interscience), New York; Butterworths, London, 1960. ix + 203 pp. Illus. $6.50.

Although moisture in textiles has been a topic of fundamental importance for many years, not all aspects of the problem have been properly appreciated by students and recent research workers. This book, based on a series of lectures given at the Manchester College of Science and Technology, provides some help in a convenient, compact form. The authors of the various chapters are well known authorities, each an expert in the field he discusses.

In the first chapter, P. S. H. Henry discusses humidity: how it varies seasonally, diurnally, indoors, and out-of-doors; how it is affected by the presence of people, furniture, and windows. A brief chapter by J. W. S. Hearle describes the structures of the principal textile fibers in use today. These two introductory chapters lay the foundation for the remainder of the book.

In an excellent chapter on sorption isotherms, A. R. Urquhart summarizes research on moisture regain and explains in nontechnical language the principal theories proposed to account for the phenomena observed. The next paper, by W. H. Rees, is an interesting and lucid discussion of the heat of absorption; the latter part of this chapter explains the thermostatic action of hygroscopic textile materials when used for clothing.

In the next two chapters, G. King reviews the various theories of moisture absorption. J. Crank follows with three chapters on the rate-of-change of moisture content, simultaneous diffusion of heat and moisture, and diffusion of moisture in fiber-forming substances. Hearle follows with a chapter on moisture and electrical properties. R. Meredith has contributed two chapters on the physical properties of textile fabrics. Hearle, in the final chapter, discusses fibers and liquid water.

The book is aimed at the level of the advanced undergraduate student, except for the two chapters by King, which require a good understanding of chemical thermodynamics. Each of the chapters provides a list of references to technical literature and thus serves as a starting point for further study. On the whole, the book provides an interesting, concise review that can be read with profit by students and research workers.

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Introduction to the Theory of Quantized Fields. N. N. Bogoliubov and D. V. Shirkov. Authorized English edition, revised and enlarged by the authors. Translated from the Russian by G. M. Volkoff. Interscience, New York, 1959. xvi + 720 pp. Illus. $17.

This book demonstrates that the effects of the Iron Curtain were not wholly evil. The rapid development of quantum field theory during the years 1945–1953 was carried out almost exclusively by Western and Japanese scientists. The work was done in a hurry, in a confused and disorderly way, as is usual in any rapidly expanding field of science. The men chiefly concerned in the work were too much preoccupied with the next unsolved problem to write a coherent account of what they had already done. The Russians, compelled by force of circumstances to stand outside the battleground, had time to consider and reflect, to organize the new ideas into a balanced and scholarly exposition. As a result, the Russians have produced the two best textbooks on field theory published during the last 10 years, the first, *Quantum Electrodynamics* (by Akhiezer and Berestetskii), and now the book under review.

The emphasis throughout this book is on mathematical precision. For the first time the details of quantum electrodynamics, renormalization, and the subtraction of divergences are presented in a style free of mathematical sloppiness. This means that the customary procedures for obtaining finite results from perturbation theory are rigorously justified, while the attempts to calculate explicitly with infinite quantities are shown to be meaningless. The limits within which the standard methods of field theory are valid have thus been precisely defined.

This is not a textbook for beginners in field theory. For beginners the book by Akhiezer and Berestetskii would be much more suitable. In this book Bogoliubov and Shirkov pay very little attention to the applications of the theory, and they do not discuss any approximation methods other than perturbation theory. The book is addressed to the expert, especially to the mathematically trained person who wishes to understand the basic principles of field theory without following the devious path along which the subject developed historically.

The first eight chapters of the book form a coherent whole, and they describe rather completely the state of field theory as it existed in 1956 before the "dispersion theory approach" became fashionable. Since the ninth and last chapter deals with the subject of dispersion relations which was developing rapidly at that time, it is on a different level. Bogoliubov's proof of dispersion relations has been added as a mathematical appendix to the English addition; it arrived just too late to be included in the Russian edition of the book. Thus, the ninth chapter is inevitably less satisfactory than the rest of the book. The detachment in space and time, which gave the first eight chapters their scholarly and unhurried style, is noticeably absent here.

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New Books

Mathematics, Physical Sciences, and Engineering

Ashford, Theodore Askounes. *From Atoms to Stars.* An introduction to the physical sciences. Holt, Rinehart and Winston, New York, 1960. 654 pp. $8.50.

Atkin, R. H. *Classical Dynamics.* Wiley, New York, 1959. 281 pp. $5.75.

Bauer, Edward L. *A Statistical Manual for Chemists.* Academic Press, New York, 1960. 166 pp. $4.75.

Bentley, K. W. *The Natural Pigments.* vol. 4 of *Chemistry of Natural Products.* Interscience, New York, 1960. 313 pp. $5.

Berl, Walter G., Ed. *Physical Methods in Chemical Analysis.* vol. 1. Academic Press. New York, ed. 2, 1960. 700 pp. $19.

Boucher, H. *Organisation et fonctionnement des machines arithmetiques.* Masson, Paris, 1960. 435 pp. NF. 70.

Brand, J. C. D., and J. C. Speakman. *Molecular Structure.* The physical approach. Arnold, London; St. Martin's Press, New York, 1960. 308 pp. $8.

Branley, Franklyn. *The Moon.* Earth's natural satellite. Crowell, New York, 1960. 114 pp. $3.50 (juvenile book).

Cable, J. Wesley. *Vacuum Processes in Metalworking.* Reinhold, New York; Chapman and Hall, London, 1960. 208 pp. $5.50.

Chapman, Alan J. *Heat Transfer.* Macmillan, New York, 1960. 464 pp. $9.

Chase, Grafton D. *Principles of Radioisotope Methodology.* Burgess, Minneapolis, 1959. 294 pp. $6.

Christiansen, G. S., and Paul H. Garrett. *Structure and Change.* An introduction to the science of matter. Freeman, San Francisco, 1960. 623 pp. $8.75.

Clauss, Francis J., Ed. *Surface Effects on Spacecraft Materials.* Wiley, New York, 1960. 419 pp. $11.50. Transactions of the first symposium on the requirements of materials for temperature-control surfaces of spacecraft and the behavior of material surfaces in space.

Daniels, Farrington, Ed. *Photochemistry in the Liquid and Solid States.* Wiley, New York, 1960. 180 pp. $6. Based on some of the papers presented at a symposium held in September 1957. These papers discuss photochemical reactions, photosensitized reactions, fluorescence, kinetic considerations, triplet state, reactions involving chlorophyll, and photoreactions in solids.

Elliott, John F., and Molly Gleiser. *Thermochemistry for Steelmaking.* vol. 1. Addison-Wesley, Reading, Mass., 1960. 304 pp. $10.50. First volume in a series that is planned to collate, consolidate, and summarize the available data pertinent to the physical chemistry of steelmaking. The work was carried out at Massachusetts Institute of Technology and was supported by the American Iron and Steel Institute.

Feigelson, E. M., *et al. Calculation of the Brightness of Light in the Case of Anisotropic Scattering.* Translated from the Russian. Consultants Bureau, New York, 1960. 104 pp. Transactions of the Institute of Atmospheric Physics, No. 1, 1958.

Foust, Alan S., *et al. Principles of Unit Operations.* Wiley, New York, 1960. 588 pp. $15. The preliminary draft of this book has been used for more than 2 years as a text for junior students at Lehigh University.

Goldfarb, Nathan. *An Introduction to Longitudinal Statistical Analysis.* The method of repeated observations from a fixed sample. Free Press, Glencoe, Ill., 1960. 233 pp. $5.

Gould, David F. *Phenolic Resins.* Reinhold, New York; Chapman and Hall, London, 1959. 220 pp. $5.75.

Haaser, Norman B., Joseph P. LaSalle, and Joseph A. Sullivan. *Introduction to Analysis.* vol. 1. Ginn, Boston, 1960. 733 pp. $8.50.

Handbook of Filtration. Eaton-Dikeman, Mt. Holly Springs, Pa., 1960. 125 pp.

Harris Lawson P. *Hydromagnetic Channel Flows.* Technology Press and Wiley, New York, 1960. 96 pp. $2.75.

Harrison, Thomas R. *Radiation Pyrometry and Its Underlying Principles of Radiant Heat Transfer.* Wiley, New York, 1960. 246 pp. $12.

Hille, Einar. *Analytic Function Theory.* vol. 1. Ginn, Boston, Mass., 1960. 319 pp. $6.50.

Jones, James B., and George A. Hawkins. *Engineering Thermodynamics.* An introductory textbook. Wiley, New York, 1960. 742 pp. $8.50.

Reports

Permeability of Frog Skin to Choline

Abstract. An inward movement of choline can be demonstrated across isolated skins of *Rana pipiens* when the outside is bathed by a saline made with choline chloride. This is in contrast to an earlier report indicating that skins of *R. esculenta* are impermeable to choline. The flux was very small and independent of the active transport of sodium by the skins. For most experiments choline can be used as an "inert" replacement for sodium in this preparation.

In studying sodium movement across isolated frog skins it is frequently necessary to vary the concentration of this ion in the bathing solutions. This is often done by replacing the sodium with choline (*1*) or magnesium (*2*). Although there is little reason in many experiments to prefer either one, the use of isotonic magnesium chloride introduces a chloride asymmetry that is sometimes undesirable.

In the original work with choline an attempt was made to assess the magnitude of any choline movement across the skin by using a sensitive procedure involving acetylation followed by bioassay of the acetylcholine produced. No flux of choline could be detected across the skins of *Rana esculenta* by this method. Recently, however, the impermeability of the skin (*R. pipiens*) to choline has been questioned, and it was even suggested that the compound might be actively transported in the same direction as, and possibly in competition with, sodium (*3*). Choline is used routinely in this laboratory in experiments the design of which requires that it be virtually, if not completely, nonpenetrating. Moreover, it is being used for similar work in other laboratories (*4*). For this reason the permeability of frog skin to choline was reinvestigated, this time with C^{14}-labeled choline.

The experimental design was reasonably straightforward. Two ventral abdominal skins were set up in chambers of the type described by Ussing and Zerahn (*5*). One of these was short-circuited, and the current generated was monitored intermittently. The other preparation was not short-circuited; the skin potential was measured in this case. Both skins were from *R. pipiens*, the animals having been stored for about a month in a cold room (5°C). The labeled choline (N-methyl-C^{14}) was purified by extraction with isopropyl alcohol. The excess alcohol was removed with ether, and the choline was dried by exposure to a stream of dry air. A choline Ringer's solution was made up with nonlabeled choline treated in the same way. The isotopic compound (about 10 μc) was added, and this solution was used to bathe the outside of the skins. The inside of each skin was bathed by normal Ringer's solution. Samples of the inside solution were taken at hourly intervals, plated, dried, and counted with a gas flow tube with an ultrathin end window.

The possibility that choline and sodium might compete for the same transport mechanism was tested in the same experiment. After the inside solution had been sampled for 3 hours, 2.5 ml of normal Ringer's solution was added to the choline Ringer's in the outside chamber. This made the sodium concentration 10 mmole/lit., which nearly saturates the transport mechanism (*1*). If choline is transported by the sodium pump the choline flux should fall nearly to zero within the first minutes after the addition is made. Samples were taken for the next 2 hours and assayed as above for C^{14}.

The results of this experiment are shown in Fig. 1. Two conclusions may be drawn from the data. First, there is a movement of choline across both skins. Its magnitude is small, approximately the same as the outward *diffusion* of sodium across such preparations. The "permeability constant" for short-circuited skins in two such experiments was 8.8×10^{-4} cm/hr and 4.5×10^{-4} cm/hr. This compares with a value of 8×10^{-4} cm/hr for sodium (*1*). It might be noted that the values cited for choline probably represent upper limits for penetrability. Justification for this surmise is based in part on the tendency for choline to undergo radiolysis during prolonged storage. Any uncharged compounds produced (methanol, for example) would probably diffuse more rapidly than choline, and hence the C^{14} flux would be larger than the real choline movement. In addition, the skins used had very low diffusion resistances. The d-c resistances were about 1500 ohm cm^2 with choline Ringer's in the outside solution in contrast to the usual value of 3500 to 5000 ohm cm^2.

The second general conclusion is that choline and sodium move independently through the skin. In this experiment it can be seen that in the short-circuited skin choline influx was little changed by the initiation of sodium transport. The decrease in choline influx noted during the last period for the open-circuited skin may represent a real diminution of the flux. Initiation of a sodium influx generated the usual potential difference across the skin (40 mv, inside positive), and since this would mediate against inward cation movement some decrease in choline influx is to be expected. In neither case, however, is there any sign of interaction between the choline and sodium movements. Actually, the fact that choline movement is small and probably passive was already apparent from the results of our earlier work. It was shown that with choline Ringer's on the outside and normal Ringer's on the inside of a short-circuited skin a small *outward-directed* current developed. Since this

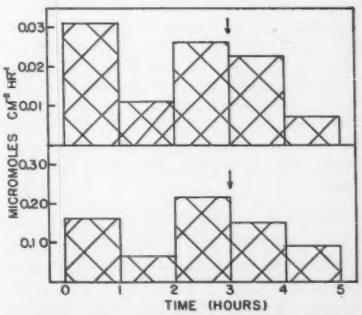

Fig. 1. Choline influx across skins of *R. pipiens*. The upper part of the figure shows the hourly movement of choline across an *open-circuited* skin. The lower portion shows the movement across a short-circuited skin. The outside of each skin was exposed to choline Ringer's during the first 3 hours. At the arrow sodium Ringer's was added to make the final sodium concentration 10 mmole/lit. Note the difference in scales on the ordinate.

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

represented a difference between choline influx and sodium outflux it was obvious that choline movement, if any, must be small and probably passive.

Thus, Koblick's contention that skins of *R. pipiens* are not impermeable to choline has been confirmed. The reasons for the discrepancy between our earlier work and these experiments are unknown. Possibly a species difference is involved, since skins of *R. esculenta* were used in the older experiments. On the other hand, the magnitude of the choline flux is very small, and for most purposes the ion can be considered to be virtually nonpenetrating, especially if skins offering average resistance to diffusion are used. There is no evidence for an active choline transport across the skins tested (*R. esculenta* and *R. pipiens*) (*6*).

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4. F. Morel, *La Méthode des Indicateurs Nucleaires dans l'Etude des Transports Actifs d'Ions* (Pergamon, Paris, 1959), pp. 155–184.
5. H. H. Ussing and K. Zerahn, *Acta Physiol. Scand.* **23**, 110 (1951).
6. This work was supported by funds provided for biological and medical research by the State of Washington Initiative Measure No. 171, and by a grant-in-aid (G-4925) from the National Science Foundation.

14 March 1960

Isolation of Antibodies to Gelatin from Antigen-Antibody Complex by Proteolysis

Abstract. Antibodies were isolated by digestion of the gelatin-antigelatin complex with collagenase and removal of the gelatin fragments by dialysis. The pure antibody could be precipitated anew by the antigen. Antibodies to tyrosylgelatin were isolated in a similar way, but in this case peptides containing the active antigenic sites remained bound to the antibody.

Antibody preparations of high purity are a prerequisite for the study of the mechanism of antibody formation and mode of action. The purification of antibodies is based either on physicochemical procedures or on immunospecific reactions. By the second method antibodies have been purified with the help of insoluble antigens, as well as by the elution of antibody from antigen-antibody complexes by heat, strong salt solutions, alkali, or acids (*1*). Antibodies have also been purified by precipitation with slightly modified antigens, followed by dissolution in alkali or acid and selective precipitation of the modified antigen (*2*, *3*).

Enzymatic cleavage of the antigen in the antigen-antibody complex would be a very convenient method for the isolation of antibodies, if such treatment would not affect the antibody molecules. In the case of diphtheria and tetanus toxin-antitoxin floccules, digestion with pepsin or trypsin yielded preparations of antitoxins of high activity, but their molecular weights were reduced to about half the original size (*1*). In this report (*4*) we describe the isolation of pure, undamaged antibodies to gelatin by selective digestion of the antigen in the gelatin-antigelatin precipitate by collagenase. This enzyme degrades collagen and gelatin to products of an average molecular weight around 500, but seems to be inactive against other proteins (*5*).

In a control experiment, gelatin (U.S.P. granular, Fisher) was digested with collagenase (*6*) (4 units of collagenase per milligram of gelatin) in 0.05*M* tris buffer, *p*H 7.4, containing 0.005*M* $CaCl_2$, at 25°C, for 3 hours. The reaction mixture was subjected to exhaustive dialysis against water at 2°C for 3 days, and the solution both inside and outside the dialysis bag were hydrolyzed (6*N* HCl, 110°C, 24 hr). All the hydroxyproline of the original gelatin was found (*7*) on the outside, while none remained in the dialysis bag. This shows that the gelatin was split quantitatively into dialyzable fragments. When human γ-globulin was treated with collagenase under similar conditions but in phosphate buffer, no dialyzable peptides were split off (Kjeldahl nitrogen analysis). It appeared, in fact, that the γ-globulin molecules were completely unchanged by the enzyme, as the material sedimented at the same rate before and after treatment with collagenase in tris buffer ($S_{20} = 6.6$, at a 0.17 percent concentration in 0.15*M* NaCl; Spinco model E ultracentrifuge, at 56,100 rev/min), and the area enclosed by the gaussian curve in the sedimentation pattern remained constant.

Rabbit antiserum to gelatin was prepared and the antibodies were precipitated with gelatin, according to Maurer (*8*). The precipitate resulting from the addition of 0.65 mg of gelatin to 13 ml of antiserum was washed repeatedly with 0.15*M* NaCl. It contained 0.25 mg of antigen (33 μg of hydroxyproline in the hydrolyzate) and 1.53 mg of antibody (derived from the extinction at 280 mμ of the neutralized solution of the precipitate in 0.1*N* NaOH). A suspension of this precipitate in 2 ml of 0.05*M* tris buffer, *p*H 7.4, containing 0.005*M* $CaCl_2$ was treated with collagenase (3 units), at 25°C. After 3 hours the remaining insoluble material

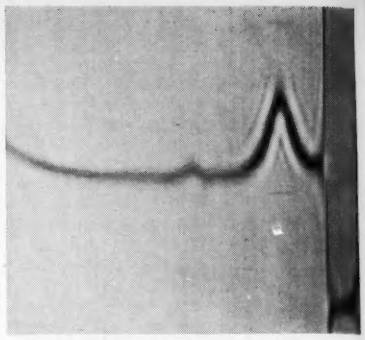

Fig. 1. A sedimentation pattern, in a Spinco model E ultracentrifuge, of anti-tyrosylgelatin (0.17 percent in 0.15*M* NaCl), at 20°C. The photograph was taken 16 minutes after full speed (56,100 rev/min) was attained.

was centrifuged off. It contained 26 percent of the gelatin in the original precipitate (65 μg of gelatin, as calculated from hydroxyproline data). The supernatant fluid was subjected to exhaustive dialysis against the buffer, at 2°C. After 3 days the contents of the dialysis bag were brought up to 3.5 ml. The fact that hydroxyproline was found in the hydrolyzate of this solution, demonstrated that no gelatin-split products were present. The solution contained 80 percent of the antibody in the original precipitate (0.35 mg of antibody per milliliter, as calculated from the extinction at 280 mμ). Only one spot was detected on paper electrophoresis, migrating identically with the γ-globulin in rabbit normal serum. The material showed a sedimentation constant of S_{20} equal to 7.14 in the ultracentrifuge [rabbit γ-globulin has a sedimentation constant S_{20} approximately equal to 7 (*1*)]. The purified antibody could be precipitated (*8*) anew by the addition of gelatin.

Gelatin is a weak antigen; it yields antisera of low antibody titer, and the precipitin reaction takes a prolonged time. Attachment of tyrosine peptides to gelatin converts it into a relatively powerful antigen (*9*). In view of this enhancement of antigenicity, the possibility of obtaining pure antibodies from the system polytyrosylgelatin-antipolytyrosylgelatin was also investigated. A gelatin derivative enriched with only 2 percent tyrosine (*10*) ("tyrosylgelatin") served as antigen in this experiment. It had been shown previously (*9*) to be a strong antigen in comparison to gelatin. Upon digestion with collagenase and exhaustive dialysis (conditions similar to the control experiment with gelatin) tyrosylgelatin was split into fragments that dialyzed out completely, as followed spectrophotometrically (293.5 mμ, *p*H 13), as

well as by the hydroxyproline analysis of the hydrolyzates (*7*).

The precipitate obtained by the addition of 3 mg of tyrosylgelatin to 25 ml of homologous rabbit antiserum (*9*) (the washed precipitate contained 1.9 mg of antigen and 8 mg of antibody) was treated with collagenase (15 units) as in the case of the gelatin-antigelatin floccules. The suspension became almost completely clear during the enzymic reaction. The fraction that dialyzed out accounted for 90 percent of the hydroxyproline present in the precipitate. The dialysis bag contained 85 percent of the antibody in the original precipitate (as calculated from the extinction at 280 mμ), as well as the remaining 10 percent of hydroxyproline. When subjected to paper electrophoresis, the antibody solution showed one spot only, with the mobility of normal rabbit serum γ-globulin. On sedimentation (Fig. 1) the solution was shown to contain a principal component with S_{20} equal to 7.06 and small amounts (approximately 6 percent) of a component with S_{20} equal to 19.2 [a 19 *S* component has been reported to be present in small amounts in many γ-globulins (*1*)]. Upon addition of new tyrosylgelatin to the antibody solution there was almost no precipitation, even when care was taken to inhibit any collagenase activity still present by the addition of Versene (*5*).

In the absence of the respective antibodies, collagenase converts both gelatin and tyrosylgelatin quantitatively into dialyzable fragments. Nevertheless, the possibility could be envisaged where the digestion of the antigen-antibody complex might leave the active site of the antigen bound to the antibody. With gelatin this does not seem to be the case, as the isolated antibody contained no hydroxyproline and could be precipitated with gelatin. On the other hand, in the case of tyrosylgelatin, the purified antibody still contained some hydroxyproline (it would correspond to 3 percent gelatin impurity, if calculated on the base of the composition of the whole antigen molecule) and could not be precipitated by the homologous antigen. This is consistent with the assumption that peptides containing the active site of the antigen are still bound to the antibody molecules. The above argument is further supported by the results of inhibition experiments of the specific precipitin reaction by means of collagenase digests of the antigens. While the collagenase digest of gelatin does not inhibit the precipitation of antigelatin by gelatin (*11*), it was found that the collagenase digest of tyrosylgelatin (the solution that dialyzed out from a mixture of 2 mg of tyrosylgelatin and 6 units of collagenase, after 3 hours of incubation at 25°C) completely inhibited the precipitation of antityrosylgelatin from 0.5 ml of antiserum, by the homologous antigen. It may be thus concluded that the hydroxyproline in the purified antibody solution is derived from fragments of the antigen bound to the active sites of the antibody molecules.

The method used here for the antibody purification is limited to systems where an enzyme may digest the antigen (protein or polysaccharide) in the antigen-antibody complex without damaging the antibody molecules. The active site of the antigen may under these circumstances remain, in some cases, bound to the antibody. This might permit the isolation of the active fragment (*12*).

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8 January 1960

Electronic Structure and Nicotine-like Stimulant Activity in Choline Phenyl Ethers

Abstract. Frontier electron density at the ether oxygen position and superdelocalizability at the ortho position show good parallelism with biological activity—that is, the stimulant activity of phenyl ether choline molecules. The mechanism of the biological action is discussed in connection with this finding.

The nicotine-like stimulant activity of phenyl ether cholines varies greatly with the substituents (*1*), but the mechanism of this activity remains obscure. The idea that the electron density at some points in the molecule, rather than the presence of some particular group, is important for the pharmacological activity, has been recognized. However, no quantum-mechanical approach in understanding the mechanism has been made.

We have established the frontier electron theory as one of the quantum-mechanical theories of organic chemical reaction, and its prediction agrees with experimental results better than previous theories (*2–4*). Furthermore, the theory has been successfully applied to the problem of some biological actions of conjugated molecules, such as the carcinogenic activity of polycondensed aromatic hydrocarbons (*5*) and the plant-growth activity of benzoic acid derivatives (*6*).

In this report, the frontier electron theory is further extended to explain the nicotine-like stimulant activity of phenyl ethers of choline. The theoretical indices used in this report are (i) frontier electron density (*2*) and (ii) superdelocalizability (*3*). These were derived as the reactivity indices of two extreme cases of stabilization at the transition state, due to the charge transfer from the substrate molecule to the pseudo-π orbital (that is, a π-like orbital which, according to the theory, comes into being near to the transition state, and consists of the orbitals in the reagent and the atom to be attacked in the substrate molecule), or vice versa. Let α be the coulomb integral of a carbon atom in benzene, and let h be the energy of the pseudo-π orbital. Then the frontier electron density and the superdelocalizability correspond to the case in which h is equal to the frontier orbital energy and to the case in which h is equal to α, respectively. The larger these indices are, the more reactive the position in question is, not only in substitution or addition reaction, but also in molecular complex formation.

Superdelocalizability has the following explicit formulas according to the type of reaction:
electrophilic reaction:

$$S_r^{(E)} = 2 \sum_j^{occ} (C_r^j)^2/\lambda_j$$

radical reaction:

$$S_r^{(R)} = \sum_j^{occ} (C_r^j)^2/\lambda_j + \sum_j^{unocc} (C_r^j)^2/(-\lambda_j)$$

nucleophilic reaction:

$$S_r^{(N)} = 2 \sum_j^{unocc} (C_r^j)^2/(-\lambda_j)$$

where C_r^j is the coefficient of the rth atomic π orbital in the jth molecular orbital, λ_j is the coefficient in the equation $\epsilon_j = \alpha + \lambda_j \beta$, ϵ_j is the energy of the jth molecular orbital, and α and β are the coulomb and resonance in-

Table 1. Nicotine-like activity and the electronic structure of phenyl ether cholines.

$R-OCH_2CH_2N(CH_3)_3 \cdot Br$ R =	Frontier electron density at ether oxygen* ($f_{oxy}^{(E)}$)	$S_o^{(N)}$*	Positive charge at ether oxygen*	Relative activity on blood pressure of cats after atropine†	
				Suprarenals intact	Suprarenals ligated
3,5-Dibromophenyl	0.764	0.952	0.179	337	268
Meta-bromophenyl	0.766	0.938	0.179	370	258
Meta-chlorophenyl	0.764	0.931	0.180	220	192
Phenyl	0.768	0.911	0.180	100	100
Meta-tolyl	0.730	0.847	0.179	13.1	13.5
Para-chlorophenyl	0.723	0.911	0.186	10.4	10.1
3,5-Xylyl	0.710	0.811	0.179	5.2	5.2
Para-tolyl	0.664	0.915	0.165	0.4	0.47

* The parameters used in the calculation are the following: coulomb integral of ether oxygen, chloro, bromo, and methyl group are $\alpha + \beta$, $\alpha + 2\beta$, $\alpha + 1.8\beta$, and $\alpha + 3\beta$, respectively. The coulomb integral of carbon attached to the chloro, bromo, and methyl groups are $\alpha + 0.4\beta$, $\alpha + 0.4\beta$, and $\alpha - 0.1\beta$, respectively, and the resonance integral between carbon and chlorine, carbon and bromine atom, and carbon and methyl groups are 0.8β, 0.7β, and β, respectively. † Hey's experiment (*1*).

tegrals, respectively, in benzene. The signs $\sum^{occ}$ and $\sum^{unocc}$ denote the summation of the occupied and unoccupied orbitals, respectively. Frontier electron density is the density of the π electrons in the frontier orbital at a position in the molecule. The frontier orbitals are defined as (i) the highest molecular orbital in the ground state, in the case of an electrophilic attack; (ii) the lowest vacant orbital of the ground state, in the case of a nucleophilic attack; and (iii) both of these orbitals, in the case of a radical attack.

We examined the calculated indices at all positions in the phenyl ethers and found that the frontier electron density for electrophilic attack at the ether oxygen position ($f_{oxy}^{(E)}$), and the superdelocalizability for nucleophilic attack at the ortho (*o*) position ($S_o^{(N)}$), have very intimate correlations with the nicotine-like activity (Table 1). This suggests that the interaction of active sites of these compounds, that is, the ether oxygen and ortho positions, with an electrophilic and a nucleophilic center in the receptor, is of great significance in the pharmacological action of these compounds. Wilson and Quan (*7*), carrying out the experiment of inhibitory action of phenyltrimethylammonium derivatives with regard to the hydrolysis of acetylcholine by cholinesterase, concluded that the steric condition was a very important factor in determining the inhibitory activity. 3-Hydroxy derivative (Fig. 1*A*), and 3-dimethylcarbamoxy derivative (Fig. 1*B*) were reported to have strong inhibitory activity; hence, these compounds were assumed to have a high degree of molecular complementariness with the enzyme. These two compounds are indicated in Fig. 1, together with phenyl ether choline (Fig. 1*C*). As is clearly seen in the figure, these compounds have sterically analogous conditions (part of the heavy line). Wilson and Quan suggested the hydrogen bond for (*A*) and covalent bond for (*B*). If these bonds are correct, the reactive center in the enzyme might be nucleophilic in nature because the proton of (*A*) or the carbamate carbon atom of (*B*) could be considered to be electrophilic. The ortho position of (*C*) corresponds sterically to the proton of (*A*) or the carbamate carbon of (*B*), and it is of interest that the value of $S_o^{(N)}$, which is the measure of reactivity of the ortho position with a nucleophilic center, shows good parallelism with biological activity. At the present stage of knowledge, it is impossible to decide what groups in the nucleophilic center might be involved in the interaction with the ortho and ether oxygen positions. In this connection, the conclusion obtained by Wilson and Bergmann (*8*) that nucleophilic groups such as imidazole and tyrosyl might be present in the esteratic site is suggestive for the nature of the group in the nucleophilic center. Perhaps a carbamate carbon atom will interact with an imidazole-like group, and ether oxygen will become an acceptor of a proton of the tyrosyl group. This might relate to the reason the value of $f_{oxy}^{(E)}$ indicates a distinct parallelism with biological activity.

The values of $S_o^{(N)}$ of *p*-chloro and *p*-tolyl compounds are rather large compared with their biological activity. The introduction of a group at the para position is expected to make the compound unfavorable for the occurrence of activity as in the case of plant-growth regulating compounds (*6*). In the fourth column of Table 1, the positive charge at the ether oxygen position is listed. The values of 3,5-dibromo, meta-bromo, unsubstituted, meta-methyl and 3,5-dimethyl are nearly constant, whereas the biological activity varies remarkably in this series of compounds. Hence the charge at the ether oxygen position seems to have no intimate connection with the biological phenomena.

It is noteworthy that the existence of the heavy line part is favorable for the occurrence of activity. That is, inhibitors of acetylcholinesterase and a greater part of nicotine-like stimulant compounds possess a chain of five atoms attached to the nitrogen atom. This steric circumstance strongly resembles the "five-atom chain" rule, proposed by Ing, associated with muskaline-like activity (*9*). It is true that the many molecules having four-atom or six-atom chains exhibit nicotine-like activity. However, activity of these compounds is not so great as one in which the "five-atom chain" rule is satisfied. Furthermore, the activity of the compound having three-atom or seven-atom chains has scarcely been reported. In this connection, the spatial factor is a very important, although not the exclusively determining one (*10*).

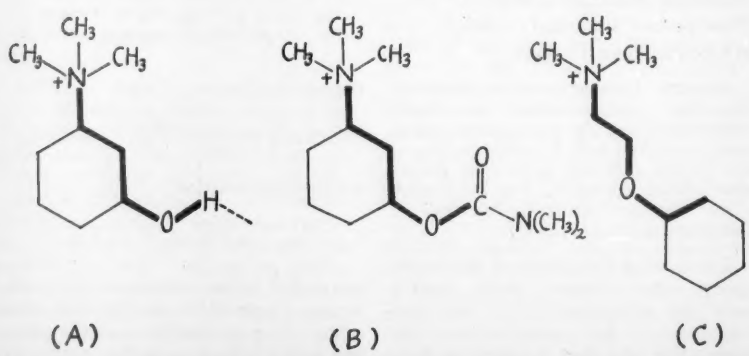

Fig. 1. Steric resemblance between the inhibitors (*A* and *B*) of acetylcholinesterase and phenyl ether choline (*C*).

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Influence of Currents on Form of Sponges

Abstract. Small reconstituted *Microciona prolifera* produced central oscular chimneys, perpendicular to the surface of attachment, in standing water; in a slow, steady current, the oscular chimneys were eccentrically placed and directed obliquely downstream. Gravity and directional illumination did not affect the orientation of the chimney.

In 1923, Bidder (*1*) pointed out that sponges of the same species differ in shape, apparently in adaptation to the water currents in which they grow; those exposed to currents of constant direction bear most of their oscula on the downstream side, while those in still water or variable currents bear oscula opening upward. Since, so far as I am aware, no attempt has ever been made to verify this suggestion experimentally, I examined the influence of certain environmental factors, including water currents, on the growth of the oscular chimney in small *Microciona prolifera* obtained by the method of H. V. Wilson (*2*), that is, by allowing cells dissociated by squeezing the sponge through bolting silk to settle on glass slides in dishes of sea water.

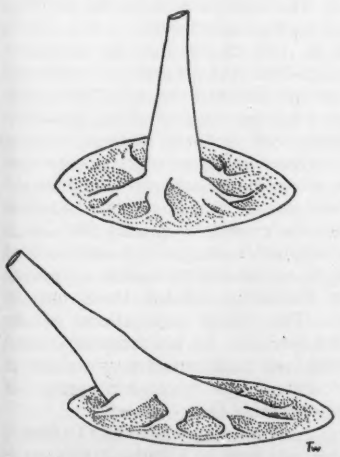

Fig. 1. Reconstituted sponges, *Microciona prolifera*, grown in standing water (top) and in slowly flowing water (bottom).

After 36 to 48 hours, before the reassociated masses of sponge cells had any detectable internal structure foreshadowing a canal system, they were well attached to the slides and could be transferred to other containers. Half the slides, with about 15 sponges, were placed in finger bowls of "standing" water—actually, kept gently turbulent by slowly dripping sea water. The other half of the slides were placed in a battery jar in which a constant, slow current was maintained by a stream of bubbles up one side.

Within a week each small sponge, 1 to 2 mm in diameter, had formed a system of excurrent canals radiating from the base of a tall oscular chimney. In standing water, every sponge produced a central chimney perpendicular to the slide on which it grew (Fig. 1, top). In flowing water, every sponge produced an eccentric chimney, pointing downstream at an angle of about 45° with the slide (Fig. 1, bottom). The same form was assumed whether the sponge grew on the upper surface of the slide, or on the lower surface, or on the vertical surface of a slide standing on edge, and whether illumination came from above, or from one side, or through the slide from below.

When a slide bearing such adapted sponges was transferred to the other kind of container, the sponges gradually transformed themselves to the form appropriate to the new conditions over a period of about 10 days. Except for a temporary shortening and constriction of the oscular chimney, the canal system seemed to remain functional throughout the transition.

These results agree well with Bidder's hypothesis that a growing sponge adopts a form which minimizes the quantity of exhaled water re-entering its incurrent pores (*3*).

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4 March 1960

Uterine Vascular Clamping: New Procedure for the Study of Congenital Malformations

Abstract. Clamping the uterine blood vessels of the rat on the 9th day of gestation for ½ to 3 hours resulted in fetal death, growth retardation, and severe congenital malformations. This procedure has broad applications in the fields of teratology and cancer chemotherapy, and in the investigation of fetal-maternal physiology.

Mechanical or operative techniques in teratology have never been widely adopted, because, in general, they are not potent teratogenic agents, or they have only minimal application to the general field of congenital malformations. The following operative procedure, developed in our laboratory, is a potent teratogenic agent and should find wide use in all phases of experimental teratology, since one animal provides both the control and experimental fetuses.

An inbred strain of pathogen-free rats with an extremely low rate of random malformations was used in the study. The rats were subjected to a 12-hour mating period, and vaginal smears were used to determine pregnancy. The first day of a positive smear was used as day zero in calculations of gestational age.

On the 9th day of pregnancy laparotomy was performed with pentobarbital anesthesia. The number, location, and condition of the implantation sites were recorded. One horn of the uterus was then clamped at its cervical and ovarian ends, the clamps extending across the uterus and its mesentery. By this means, one horn was completely isolated from the maternal circulation. The other horn served as a control. There were five rats in each of the six time periods of ½, 1, 1½, 2, 2½, and 3 hours. After the specified clamping interval, the hemostats were removed, and the abdomen was closed. The rats were killed on the 21st day.

Complete isolation of the clamped uterus from the maternal circulation was an essential condition of this experimental procedure. Intravenous injections of trypan blue and fluorescent dyes qualitatively confirmed uterine hemostasis, since no dye was noted on the clamped side. A more quantitative method was obtained by injecting 12.48 μc of radioiodinated albumin intravenously, after clamping a (nonpregnant) uterus. The two horns were then excised and divided into three segments. The counts per minute per milligram of tissue showed negligible radioactivity in the clamped segments (Fig. 1*a*).

Either of two reactions followed clamping: (i) a sudden arteriolar spasm

with blanching of the uterus, followed by deepening cyanosis during the next 15 minutes or (ii) gradual cyanosis, again reaching a peak between 10 and 15 minutes. The cyanosis persisted until the clamps were removed.

Uterine clamping affected the fetuses in several ways. Fetal death with resorption, or growth retardation or, sometimes, severe congenital malformations occured. Many fetuses were unaffected by the procedure, but most of these were in the shorter time intervals.

Occurrence of fetal resorption was directly related to the duration of clamping (Fig. 1*b*). Control mortality at ½ hour was 12 percent; it gradually rose, as the procedure was prolonged, to 33 percent in the 3-hour group. At ½ hour, 28 of 33 clamped embryos survived, a mortality rate of 15.1 percent. At 3 hours, 21 of 22 clamped embryos were resorbed; the one survivor was malformed.

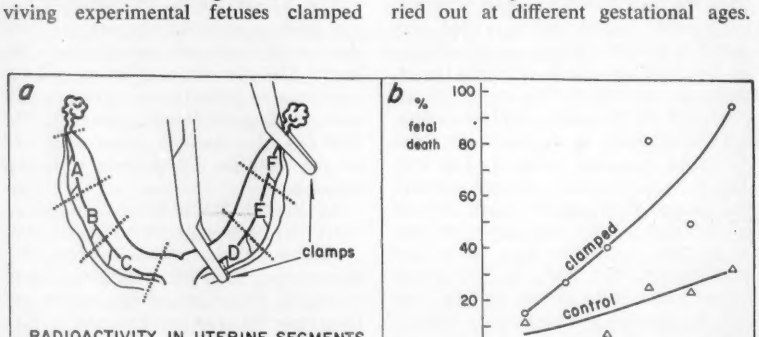
The mean fetal weights in the surviving experimental fetuses clamped 1½ hours or less were not significantly different from those of the control groups (Fig. 1*c*). The mean fetal weights of the 2- and 2½-hour experimental groups were significantly less than the weights of the corresponding control groups.

One of the 116 surviving control animals was malformed, having a right-sided aortic arch. There were no malformations among the 49 survivors in the ½- and 1-hour experimental groups. In the groups clamped from 1½ to 3 hours, 8 of the 48 surviving fetuses (16.7 percent) were malformed. The 11 malformations consisted of anophthalmia (Fig. 1*e*), microphthalmia, renal aplasia (Fig. 1*g*), renal agenesis, uterine agenesis (Fig. 1*g*), pancake adrenal, anencephaly (Fig. 1*f*), omphalocele (Fig. 1*f*), absent external ear (Fig. 1*d*), hydrocephalus, and absent fourth aortic arch (infantile coarctation).

Similar experiments have been carried out at different gestational ages. Preliminary results of clamping on the 8th day show a higher mortality and more severe malformations.

The teratogenic action of vascular clamping may be due to either, or both, of two factors: (i) a deficiency of anabolic and metabolic products utilized by the embryonic tissues, or (ii) an excess of catabolic products produced in these tissues with no communication with the maternal circulation. Whether a single factor such as hypoxia, excessive carbon dioxide, or *p*H changes is the etiologic agent is unknown at this time, but it is clear that the cause lies in the alteration produced in the intrauterine environment. Low oxygen tension would appear to be the first mechanism to be investigated, since it has been shown to be teratogenic in mice (*1*). Experimental hypoxia in the rat has so far failed to produce congenital malformations (*2*), although excess carbon dioxide has been reported to be teratogenic (*3*). Certainly, valuable information can be obtained by measurement of the gas contents and chemical changes in the tissue and vascular compartments of the clamped uterus and embryos. More quantitative data on the possible roles of hypoxia and excessive carbon dioxide can be obtained by this procedure. Furthermore, attempts should be made to reverse the effects of clamping by hyperoxygenating the mother prior to clamping, exogenous oxygenation of the clamped segment, or alterations in maternal acid-base balance before clamping.

Since there is a period during the first hour of clamping, on the 9th day, in which there are no malformations and no weight reduction, the clamped side can be employed as a control for a wide variety of experiments using the unclamped horn as the experimental side. The determination of the length of action of a teratogenic drug is an example of one application of this technique. Thus, the presence of malformations on the clamped side after injection of a teratogenic chemical would characterize the drug as to its rapidity of action and degree of synergism with the clamping procedure. Chemotherapeutic agents could be similarly tested, since the embryo is a good test animal for antitumor drugs. The variations of the procedure are numerous, and spread into the field of fetal-maternal physiology. The broad applications of the procedure as a pharmacologic and physiologic technique are as worthy of investigation as is its mechanism of teratogenesis (*4*).



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CONTROL	Number fetuses Day 9	Day 21	Weight ± S.D.
½ hour	25	22	4.54 ± .55
1 "	21	20	4.91 ± .36
1½ "	24	22	4.90 ± .61
2 "	27	20	4.91 ± .32
2½ "	25	19	4.90 ± .77
3 "	18	13	4.50 ± .86
CLAMPED			
½ hour	33	28	4.42 ± .58
1 "	29	21	4.57 ± .46
1½ "	39	23	4.50 ± .63
2 "	35	6	3.79 ± .99
2½ "	34	17	4.20 ± .64
3 "	21	1	(one animal)

Fig. 1. (*a*) Method of clamping the uterus, and radioactivity of clamped and unclamped horns. (*b*) Percentage mortality among experimental and control fetuses after various periods of clamping. (*c*) Tabulation of the fetal weights of clamped and control animals. (*d*) Anophthalmia and absent pinna in animal from litter clamped 3 hours. (*e*) Anopthalmia on left (arrow) with normal eye on right in animal from 2-hour experimental group. Skin over orbit has been removed. (*f*) Anenecephaly and omphalocele in fetus from 2-hour experimental group. (*g*) Dissection of fetus from litter clamped 1½ hours, showing aplastic left kidney (arrow) and corresponding changes in left adrenal.

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Increased Stress and Effectiveness of Placebos and "Active" Drugs

Abstract. Evidence is presented to indicate that placebos are far more effective in producing carefully defined relief of pathological pain than they are in the case of experimental pain. This is construed as further support for the view that placebos are more effective when stress is great than they are when stress is not so great. A similar situation holds for morphine. Certain drugs are effective in relieving visceral sensations only if an essential psychological state is present. This is, in effect, a new principle of drug action.

This report presents an experimental finding: placebos relieve pathological pain more effectively than they do experimental pain. Two general concepts grow out of this observation as working hypotheses: (i) the effectiveness of placebos increases with increased stress and (ii) the effectiveness of certain "active" drugs increases with increased stress.

In essence, this study is based upon the proposition that pain of pathological origin produces more anxiety, or stress, than does experimentally contrived pain. Like any axiom, this one may be unprovable in a tight mathematical sense, yet its truth is clearly evident. Even so, the material in this report is presented as evidence for, not proof of, the proposition, just as was the case in an earlier study (*1*), where material of another kind was presented. In that earlier paper it was found: (i) in terms of percentage of a given population relieved, placebos are significantly more effective when postoperative pain is severe than they are when the pain is less severe, and (ii) the work of Cleghorn, Graham, Campbell, Rublee, Elliott, and Saffran (*2*) demonstrates that firing of the adrenal glands (measured in objective terms) is far greater in response to a placebo in patients hospitalized for severe anxiety than in patients hospitalized for anxiety of less severe degree.

The new data are presented in Tables 1 and 2. The most important fact arising from these data is that the mean percentage effectiveness of placebos in relieving pathological pain is over ten times that found with experimental pain. It is realized that some of the studies compared are based on large samples and some on small, and that the *t*-test evaluation ignores this fact and gives all studies equal weight. For present purposes this is satisfactory since numerous studies are involved and the difference between the two conditions is great.

It is not my contention that placebo effectiveness is always low in experimental situations. This is demonstrably not the case. The matter is complex, and it may be in these other cases that stress of one kind or another operates when placebo effectiveness is high. It is important to deal, as here, with a limited area at a time; in the present instance attention is given solely to comparison of placebo effectiveness in relieving pain of (i) pathological and (ii) experimental origin.

Placebos, being "inert" agents, can affect only psychological processes. The assumption is that when the psychological component of a situation is important the placebo will have a correspondingly greater opportunity to produce an effect, and this seems to be the case. The primary purpose of this paper is, however, to present observations rather than speculations.

Effects similar to those of a placebo can be found with an "active" drug: morphine, even in large doses, does not dependably relieve pain of experimental origin in man, as indicated by some 15 different groups of investigators (*3*, pp. 123, 124). Morphine in comparable (or smaller) doses is highly effective in relieving pain of pathological origin. Pathology (stress, see below) provides the matrix on which the given drug (morphine), ineffective as it was in relieving experimentally contrived pain, becomes effective when the necessary component—apparently stress—is present (*3*, p. 164). Pathology alone (stimulation of pain endings in battle wounds) is often not enough to give rise to pain. The psychological significance to the subject determines the pain experienced (*3*).

The new principle is: certain drugs are effective in relieving visceral sensations only if an essential psychological state is present. It is not possible at present to define exactly the nature of this state. It appears to be related to the significance of the symptom, to anxiety, and to stress. (And apparently, the stronger the psychological state, the more effective the drugs.) Similarly, certain common symptoms, pain for example, appear to emerge only if an essential psychological state (anxiety, stress) is present (*3*). Physiological derangement (stimulation of pain end-

Table 1. Effectiveness of placebos in relieving pain of experimental origin. The numbers in parentheses in column 1 refer to studies cited in "References and Notes."

Study	Subjects (No.)	Average placebo effect (% relieved)	Comment
		Radiant heat	
(*4*)	16	0.8	Suprathreshold pain, untrained subjects
		0.4	Suprathreshold pain, semitrained subjects
		4.0	Suprathreshold pain, trained subjects
(*5*)	4	0	± 1.1% change from original value
(*6*)	3	0	Obstet. cases; radiant heat pain only
		Radiant heat to forehead	
(*7*)	1	19	1, no effect; four trials
	1		1, 20% rise; no effects 2nd and 3rd trials
	2		2, 28% rise
(*8*)	1	2	
(*9*)	29	0	No consistent trend
		Pressure on forehead	
(*10*)	4	0	Aching pain
		Pressure on forehead, cuff method	
(*11*)	63	0	Inconsistent, variable + and −
		Hydrostatic pressure in biliary system	
(*12*)	8	0	
		Electric shock	
(*13*)	30*	1.3†	Pain intensity comparison; no drug versus
		5.0‡	placebo
(*14*)	3	0	
		Tourniquet	
(*15*)	4	15.5	
(*16*)	4	0	
Totals (13 studies):			
	173	3.2 ± 1.8	Average percentage relieved

* Postaddicts; † $N = 16$; ‡ $N = 14$.

Table 2. Effectiveness of placebos in relieving pain of pathological origin. The average percentage relieved in Table 2 differs from the average percentage relieved in Table 1: $t = 9.28$ and $p = 0.0001$.

Study	Subjects (No.)	Satisfactorily relieved by placebo (%)
Severe postoperative wound		
(*17*)	118	21
(*18*)	29	31
(*19*)	34	26
(*20*) (The av. percentage relieved by placebo was 33%.)	52	40
	36	26
	44	34
	40	32
(*21*) (The av. percentage relieved by placebo was 39%.)	14	50
	20	37
	15	53
	21	40
	15	40
	15	15
Pain from angina pectoris		
(*22*)	66	38
(*23*)	19	26
(*24*)	27	38
Pain from metastatic disease		
(*25*)	67	42
Headache		
(*26*)	199	52
Totals (10 studies):	831	Av. 34.6 ± 2.9

ings) is not enough. Thus there is exposed in this new framework unsuspected ties between mind and body, revealed by the study of drug action.

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12 January 1960

A Function for Ascorbic Acid in the Metabolism of an Insect

Abstract. Homogenates of *Blattella conjuncta* oxidize L-tyrosine. The reaction is diminished by dialysis, but may be reactivated with L-ascorbic acid. Glutathione, pyridoxal phosphate, and folic acid also activate the system.

It has been demonstrated (*1–3*) that ascorbic acid will activate the L-tyrosine oxidase system of mammalian liver. However, the function of the compound in insect metabolism is unknown. Previous investigations (*4*) have demonstrated that the cockroach contains large amounts of ascorbic acid. In fact, the concentration in the whole insect (10 to 19 mg/100 gm) greatly exceeds that in whole guinea pigs [2.4 mg/100 gm (*5*)]. This concentration presumably indicates that ascorbic acid plays an important role in insect metabolism. The present study was undertaken to study tyrosine oxidation by insect tissues which, by analogy with the process in mammalian tissues, would be expected to require ascorbic acid.

Blattella conjuncta was captured locally in the Upper Hutt Valley, near Wellington, New Zealand. The adult insects were killed by decapitation, and the whole insects were homogenized with two volumes of ice-cold 0.14*M* potassium chloride solution in a Potter-Elvehjem apparatus. Insoluble materials were removed by centrifugation at 2°C. The supernatant was adjusted to *p*H 7.5 and dialyzed in a cellophane bag against 0.14*M* potassium chloride solution for 24 hours at 4°C. The resulting solution was used as the source of the tyrosine oxidase system.

Table 1. Effects of ascorbic acid and other cofactors on the tyrosine oxidase activity of a dialyzed insect homogenate. The activity is given in microliters of oxygen per hour.

Components added to homogenate	Activity
None	11
α-Ketoglutarate (α-K)	14
α-K + ascorbic acid	31
α-K + glutathione	15
α-K + pyridoxal phosphate	14
α-K + folic acid	21
α-K + ascorbic acid + glutathione	42
α-K + ascorbic acid + glutathione + pyridoxal phosphate	49
α-K + ascorbic acid + folic acid	37
α-K + ascorbic acid + glutathione + pyridoxal phosphate + folic acid	50

Activity was determined by Warburg techniques at 37°C in air with sodium hydroxide papers in the center wells of the vessels. Each vessel contained 1.0 ml of the dialyzed solution, 1.0 ml of 0.2*M* phosphate buffer at *p*H 7.2, and 6 μmole of L-tyrosine. The side arm contained 0.1 ml of 0.1*M* α-ketoglutarate and 0.1 ml of a solution of cofactors. The total volume in each vessel was 3.0 ml. The following cofactors were used in some experiments: ascorbic acid (1 mg), glutathione (1 mg), pyridoxal phosphate (10 μg), and folic acid (200 μg). The reaction was initiated by tipping the side arm. Manometric readings were taken for 1 hour.

The results of a typical experiment are given in Table 1. It is clear from these figures that the tyrosine oxidase system is activated by ascorbic acid. It is also activated by folic acid. The greatest effect results from additions of ascorbic acid, glutathione, and pyridoxal phosphate.

It is clear that the oxidation of L-tyrosine by homogenates of this insect probably follows a pathway similar to that in mammals. It is known that the latter deaminate tyrosine by a transamination reaction with α-ketoglutarate which requires pyridoxal phosphate (*6*). The resulting *p*-hydroxyphenylpyruvic acid is oxidized to homogentisic acid and carbon dioxide. This process involves 2,5-dihydroxyphenylpyruvic acid as an intermediate and requires ascorbic acid and glutathione. The effect of folic acid on the system is difficult to explain, but a similar effect has been reported for the mammalian system and it has been suggested that the stimulation is not due to a direct effect of the compound on the enzymes (*2*).

The present study clearly establishes a function for ascorbic acid in insect metabolism.

MICHAEL H. BRIGGS
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21 March 1960

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Four of us at Woods Hole

Four of us expect to put in a rugged week on Cape Cod beginning early on the morning of August 1. It's not exactly an exhibit or symposium or anything as formal as that. It's just that the management of the Marine Biological Laboratory at Woods Hole has given us to understand that if we want to hang around for five days dispensing free advice on how to use photography to communicate results of biological investigation, they will probably refrain from calling the cops to throw us off the premises.

One of us will be there to hold court on problems encountered in photomicrography and photomacrography of marine life—movies or stills, color or black-and-white. Another of us will take over when questions come up of photographing living small specimens at natural size or less, with or without flash. The third man knows a lot about photographic materials for electron micrography, autoradiography, and microradiography. The fourth, a biochemist, will make himself generally pleasant and helpful.

We figure any time spent feasting on Homarus americanus *is time lost from showing our movie of* Homarus americanus *hatching in glorious Eastman color.*

Nice sharp pictures

We must not lose our sense of proportion. We must recognize that some people take so little technical interest in photography that they have never even heard of *Kodak Tri-X Pan Film.* Others who do use it have noticed that since that "Improved Type" has appeared on the carton, results have somehow improved. Most of these folk will be content with a bland assertion that the recently added notation signifies a major advance in reduction of graininess and improvement in picture sharpness. A single congenial luncheon table could probably accommodate all who would press us to explain that what we have really done is to increase the peak value of the informational sensitivity *I* of *Kodak Tri-X Pan Film* from .0016 to .0036

where $I = \frac{g}{a_s} \cdot \frac{1}{\sigma_s(D)}$

g = gradient of the linearized characteristic curve

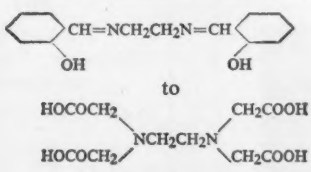
a_s = area of the spread function

$\sigma_s(D)$ = standard deviation of the granularity trace for a scanning aperture having the size of the spread function

If you want to talk like that you should first consult Journal of the Optical Society of America, 48, *926, but if you just want very sharp pictures from very fast film, ask the Kodak dealer for Kodak Tri-X Pan Film, Improved Type.*

Suggestion at Pittsburgh, Pa.

When we read the advance program for this year's Pittsburgh Conference on Analytical Chemistry & Applied Spectroscopy, we decided on a coup. One of the papers was to state an interesting fact about bissalicylideneethylenediamine, which stands in relation to the well known chelating agent EDTA as



to

When the former, in slightly alkaline *N,N-Dimethylformamide* (Eastman 5870), chelates Mg ions, it becomes highly fluorescent. The excitation maximum occurs at 355mμ, and emission maximum occurs at 439mμ. A sensitivity of 7×10^{-5} micromole of Mg per ml was mentioned.

This time, said we to ourselves, when they come charging out of the meeting room to our exhibit to ask whether we have this reagent, they will encounter no mere bland smile. We will be able to reply, "Yes, sir, here it is, labeled *α,α'-(Ethylenedinitrilo)di-o-cresol* and designated Eastman 8019."

And that is exactly what we were in a position to do that day. The only trouble was that while many people did flock to our booth and talked of many things, not one soul as much as mentioned bissalicylideneethylenediamine.

Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company) has some 3800 other Eastman Organic Chemicals. Many of them do excite a decent level of interest.

Spectroscopy in electronics

We wish we could inspire several dozen more persons to enter the craft of emission spectrography. They would become customers for *Kodak Spectrum Analysis Plates* and *Films* as well as occupants of a secure place in technical society, one of waxing importance.

To convince that the importance indeed waxes we could send copies of a disquisition written by one of our dealers, a gent willing to undertake some deep thinking and digging out of useful information in hope of the favor of an order.

Think of the old days, he suggests, when electronics meant radio and the man at the end of the radio assembly line was given plenty of adjustable resistors, capacitors, and coils on the chassis to adjust in compensation for the unpredictable characteristics of the vacuum tubes. Electronics isn't that way any more, he implies. Today electronics is supposed to assume that its solid-state devices and the cathodes of its vacuum tubes will behave predictably within very narrow limits.

And what sets these limits?

Among other things, the presence or absence of certain chemical elements in the range of parts per billion.

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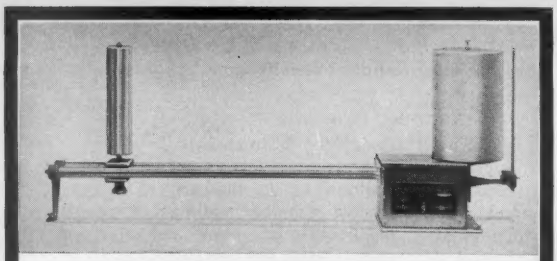

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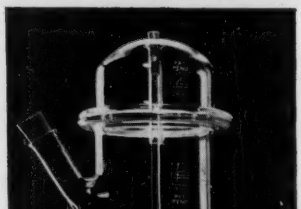

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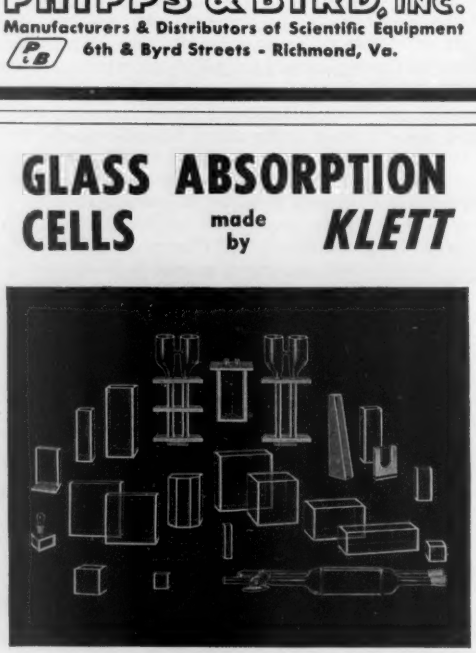

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Letters

Oxygen Transport

I found Scholander's article on O_2 transport [*Science* **131**, 585 (1960)] very interesting, and I think I can lend support to it since similar conclusions were drawn (though not published) by myself and J. P. Baumberger some years ago in a completely different experimental situation [*J. Gen. Physiol.* **36**, 255 (1952)].

We found that the polarographic diffusion current for O_2 given by whole blood was six times greater than that given by plasma when both were measured at O_2 tension of 100 mm-Hg at 37°C. This excess diffusion current depends not only on red cell concentration but on factors which increase the rate of dissociation of HbO_2: degree of O_2 saturation, pCO_2, pH, and temperature. The readiness with which HbO_2 releases O_2 determines (i) the rate at which O_2 is replaced in the immediate vicinity of the "consuming" cathode and (ii) the rate at which Scholander's "bucket-brigade" operates, at a greater distance from the cathode. (The rate of "passing the bucket" obviously depends on the dissociation rate.) The driving force behind this steady-state flow is the constant rate of O_2 "consumption" of the cathode, which maintains a constant pO_2 gradient in its vicinity.

The resultant of this situation is that, at a pO_2 of 100 mm-Hg, the cathode receives O_2 from whole blood at a rate at which it could receive O_2 from plasma only at a pO_2 of 600 mm-Hg. If we now substitute rapidly metabolizing cells for the cathode, the conclusion follows that in the presence of oxygenated red blood cells the effective rate of diffusion of O_2 to the consuming cells at a given pO_2 will be much greater than could be expected on the basis of the O_2 tension difference alone.

GABOR MARKUS
Roswell Park Memorial Institute, Buffalo, New York

Competitive Exclusion

As an economist, I have been delighted with the recent revival of the earlier view that ecology and economics are closely related. Hardin's article ["The competitive exclusion principle," *Science* **131**, 1292 (29 Apr. 1960)] is an excellent example of this revival, as would be expected in view of his previous work. Unfortunately, it contains an error in economics. From the principle that complete competitors cannot coexist he deduces the development of monopolies. The principle, however, applies to species, not individuals. Park's experiments did not show that one individual flour beetle grew so large that it eliminated all the others, only that one species grew so numerous that it eliminated the other. If the principle has any application to economics at all, it would indicate that one type of economic enterprise might, by multiplication of its members, replace another, but this would not lead to monopoly. The problem of monopoly is a real one, and an important one, but it has nothing to do with the competitive exclusion principle.

I rather dislike attacking an article which is, in general, as superior as Hardin's, but in this one area he is wrong.

GORDON TULLOCK
Department of International Studies, University of South Carolina, Columbia

Garrett Hardin points out that the "competitive exclusion principle"—that is, that "complete competitors cannot coexist"—is essentially a theoretical concept unlikely of direct proof in the field. I believe this conclusion is strengthened if it is considered that at least three common conditions, not mentioned by Hardin, must be *absent* if the replacement of one species by its complete competitor is to proceed in a direct fashion. However, at least two of these conditions are likely to be *present* in the field unless the replacement process is quite rapid.

If there exist two complete competitors, species A and B; if species A is slowly replacing species B; and if this process is to continue directly to the point of (local) extinction of species B; then the following possibilities must *not* occur:

1) A decrease in the genetically determined competitive ability of species A below the level of that of species B.

2) An increase in the genetically determined competitive ability of species B above the level of that of species A.

3) A change in the environment sufficient to shift the competitive superiority from species A to species B.

The first possibility is somewhat unlikely, on evolutionary grounds; the second is fairly likely if the replacement process is quite slow, a condition which also implies that the difference in competitive abilities of the two species is small and hence likely to be easily changed by genetic variation. The third possibility would appear to be quite likely under many conditions. If the particular environmental factor is one which oscillates between the condition favoring one species at one time and the other at another time, and if the rate of oscillation is sufficiently fast as com-

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pared to the rate of replacement of either species by the other, then the two complete competitors may, in fact, continue to coexist for a long period of time.

WERNER G. HEIM
Department of Biology, Wayne State University, Detroit, Michigan

If one thinks of a corporation as an individual, Gordon Tullock's criticism is justified. But this "model," though sanctified by a century's evolution of U.S. legal theory, is not the only possible one. One can also think of a corporation as an aggregate of individuals competing with other aggregates engaged in the same line of business. We assume no interbreeding of the aggregates ("mergers"). The equivalent of biological reproduction may be taken to be the hiring of new personnel. The limit of possible income is the limit of consumer demand for the goods or services of the kind offered. If there is free competition and no ecological differentiation, the most efficient aggregate will necessarily displace all others.

A tendency toward this sort of displacement is seen also in the competing of any two "species" of cells within the same individual whenever there is a breakdown of the poorly known cybernetic controls that keep the various kinds of tissues within bounds. See, for instance, G. Crile's review of the cancer problem [*Perspectives in Biol. and Med.* **3**, 358 (1960)]. Within a multicellular body that must meet certain stringent demands of the external environment, the exclusion principle cannot, of course, be worked out to its conclusion; the multicellular envelope dies first.

Werner G. Heim's remarks point up some important points which were scarcely more than hinted at in the last section of my article. We now know of many competing species, or competing alleles within a species, that manage to coexist because their relative competitive efficiencies change with the seasons, and the seasons always change. N. W. Timofeef-Ressovsky [*Biol. Zentr.* **60**, 130 (1940)] has carefully described the seasonal alternation of genotypes in a beetle. Comparable studies have been made with other species by E. B. Ford in England and T. Dobzhansky and his students in this country.

In addition, our theory must take account of changes in the environment that are brought about by organisms themselves. M. J. Beijerinck's "enrichment culture" method [see F. Stockhausen, *Okologie, "Anhäufungen" nach Beijerinck* (1907)] is a direct application of the competitive exclusion principle to the problem of securing a nearly pure culture of the wanted species from a very mixed natural culture. But the method is limited by the fact that, in general, any species multiplying in a closed system will tend to make the environment less favorable for its own way of life, and thus more favorable for other forms. The result of a succession of such alterations is "ecological succession."

Facts such as these do not undermine the principle; rather, their explanation (when achieved) enriches the theory.

GARRETT HARDIN
University of California (Santa Barbara), Goleta

Names for the Sun and the Moon

The Future Scientists of America Science Club of Bergenfield, N.J., propose [*Science* **131**, 380 (1960)] the proper names *Sol* and *Luna* as substitutes for the better-known *sun* and *moon* on the ground that the latter words are common nouns and not proper ones. While the argument is plausible, I think it should be pointed out that logical reasons can be adduced for *sun* and *moon*, that the question is not a scientific one but one of English usage, and that some of the assertions are too strong.

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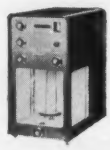

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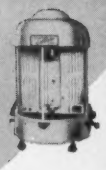

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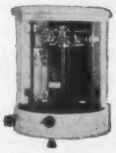

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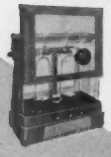

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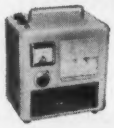

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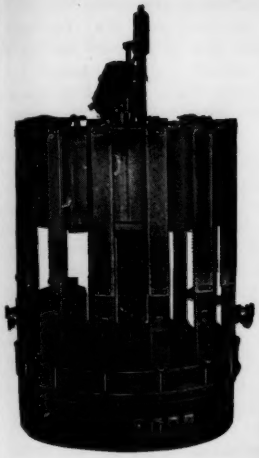

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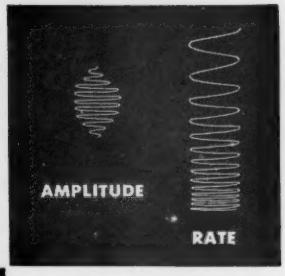

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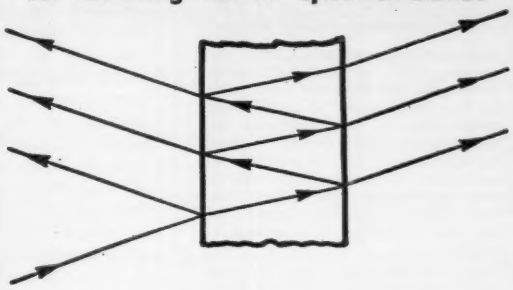

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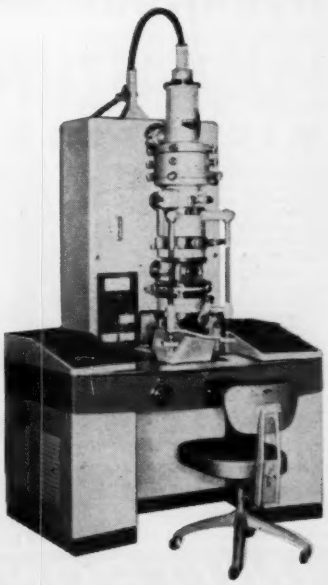

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To my knowledge, no English-speaking person ever refers (excepting in poetic or heroic utterance) to "sun" or "moon"; it is always "the sun" or "the moon." Use of the definite article indicates the object without ambiguity, whether the names are capitalized or not. We see the same usage in the terms *the nation, the president*, and especially *the earth, the world*, and *the galaxy*. It is equally possible to speak of "a sun" or "a moon" or "a nation" (but not so easily of "an earth"), and every person moderately familiar with English understands what is meant. The logic of the matter is that the definite article is often as good as a special name and is almost always used in place of the latter whenever it is not ambiguous—for example, "the sky," "the ocean," and (in the family) "the newspaper" and "the car." Furthermore, there is nothing wrong with capitalizing *Sun* and *Moon* if anyone wishes to do so; it is done in some astronomical publications, for consistency with *Venus* and *Jupiter*, and to avoid very frequent use of *the*.

The experience of centuries has shown that usage cannot be governed by fiat. Educated persons are going to be guided by dictionaries, and dictionary-makers, by literary usage, not by pseudo-scientific jargon. It is not possible for a group of scientists to reform the language to the extent proposed, even if they should be generally agreed on the desirability of doing so. I urge all serious students of science to devote their energy to the subject, rather than to hopeless causes.

Sol and *Luna* cannot rightly be said to be established names, excepting perhaps in science fiction; the "reference material" available to Vincent Massaro is not entitled to as much regard as are standard dictionaries and encyclopedias. Fowler, in his *Modern English Usage*, calls *Sol* a sobriquet, "a thing to be avoided"; he does not mention *Luna*. The words *sun* and *moon*, at least when prefixed by *the*, do not convey a "vague notion" to "people throughout the world." Such exaggerations are better avoided when one is exhorting scientists, for they have an effect opposite from the one intended.

G. M. CLEMENCE

Washington, D.C.

The Future Scientists of America Science Club decided to differentiate our sun and our moon from other suns and moons by giving them special names—*Sol* for the sun and *Luna* for the moon.

There can be no doubt that it is necessary to have special names for our sun and for our moon. It is true that "sun" in general must be distinguished from the sun that is the center of our planetary system.

The Future Scientists of America attempt to introduce the word *Sol* as a name for our sun, and they overlook the fact that this is a word not only from the extinct Latin language but also from the very vital Portuguese and Spanish languages. Not less than 20 nations on the American continent speak Spanish or Portuguese, and their *sol* corresponds to the English *sun* in all respects. So, when a specialized alternative for the English *sun* is necessary, then it is just as necessary for the Spanish and Portuguese *sol*.

I think that new words introduced in science must be acceptable to as many nations as possible. This cannot be achieved by giving a frequently used word from one group of languages a special meaning in another. It must be confessed that such specialized definition has occasionally occurred in the past. However, it seems to me unique that such a popular and common word as the Spanish-Portuguese *sol* should be suggested as a stopgap in an international science such as astronomy.

WILFRIED H. PORTIG

Department of Meteorology, University of Texas, Austin

Massaro regrets the lack of proper names for the sun and moon of the earth. He recommends, for *international* use, the words *Sol* and *Luna* as proper names for these two bodies in our system, the words *sun* and *moon* to be retained as common names for the center of a system and a satellite in general. Unfortunately, Massaro considers the problem solely from the point of view of his own language, in which *Sol* and *Luna*, being different words from the common designations *sun* and *moon*, may easily be established in the usage suggested. But what about other languages? For instance, *sol* is the common name for sun in Danish, Swedish, and Norwegian, and the Russian common name for moon is *luna*.

Being no authority on the question myself, I should not have written this letter save for the fact that Massaro is not the only person to take such an attitude to linguistic qustions of international interest. Thus, in the recent discussion in *Science* on the problems connected with transliteration of Russian texts, the proposals advanced were largely based on English pronunciation, irrespective of the fact that certain letters or combinations of letters sound quite different in other languages. In deliberating such problems with a view to putting forth recommendations for international usage, those concerned would be well advised to consider carefully the suitability of their proposals in different languages.

S. ROZENTAL

Nordisk Institut for Teoretisk Atomfysik, Copenhagen, Denmark

Meetings

Forthcoming Events

August

1–12. Modulation Theory and Systems, Cambridge, Mass. (E. J. Baghdady, Dept. of Electrical Engineering, Massachusetts Inst. of Technology, Cambridge)

2–5. Poultry Science Assoc., Davis, Calif. (C. B. Ryan, PSA, Dept. of Poultry Husbandry, Texas A & M College, College Station)

3–6. Gas Chromatography (Infrared Spectroscopy Inst.), Nashville, Tenn. (N. Fuson, Fisk Infrared Inst., Fisk Univ., Nashville 8)

3–6. Rarefied Gas Dynamics, 2nd intern. symp. (by invitation only), Berkeley, Calif. (Engineering and Science Extension, Univ. of California, 2451 Bancroft Way, Berkeley 4)

5–6. Pennsylvania Acad. of Science, summer annual, Grantham, Pa. (K. B. Hoover, Messiah College, Grantham)

6–12. International Geographical Cong., 19th, Stockholm, Sweden. (IGC, Postfach, Stockholm 6)

7–10. American Soc. of Clinical Hypnosis, Miami, Fla. (S. Hershman, 6770 N. Lincoln Ave., Chicago 46, Ill.)

7–12. Gerontology, 5th intern. cong., San Francisco, Calif. (L. Kuplan, Intern. Cong. of Gerontology, P.O. Box 2103, Sacramento 10, Calif.)

7–13. Industrial Research Conf., Harriman, N.Y. (Miss M. F. Garvey, Industrial and Management Engineering Dept., Columbia Univ., New York 27)

8–11. American Astronautical Soc., Seattle, Wash. (R. M. Bridgforth, AAS, Propulsion Unit, Boeing Airplane Co., Aero-Space Div., P.O. Box 3707, Seattle)

8–12. American Inst. of Electrical Engineers, San Diego, Calif. (R. S. Gardner, AIEE, 33 W. 39 St., New York 18)

8–13. World Federation for Mental Health, 13th annual, Edinburgh, Scotland. (Secretariat, WFMH, 19 Manchester St., London, W.1, England)

8–20. American Soc. of Criminology, London, England. (D. E. J. MacNamara, New York Inst. of Criminology, 115–117 W. 42 St., New York 36)

9–13. Hail Storms, intern conf., Verona, Italy. (H. G. M. Ligpa, American Meteorological Soc., Stanford Research Inst., Stanford, Calif.)

11–13. Rocky Mountain Radiological Soc., Denver, Colo. (J. H. Freed, 4200 E. Ninth Ave., Denver 20)

11–16. Canadian Teachers Federation, Winnipeg, Manitoba. (G. G. Croskery, 444 MacLaren St., Ottawa 4, Ontario)

14–19. American Pharmaceutical Assoc., Washington, D.C. (R. P. Fischelis, APA, 2215 Constitution Ave., NW, Washington 7)

14–19. International Cong. of Clinical Chemistry, Edinburgh, Scotland. (S. C. Frazer, Clinical Laboratory, Royal Infirmary, Edinburgh)

14–20. Cardiology, 6th Inter-American cong., Rio de Janeiro, Brazil. (H. Alqueres, P.O. Box 1594, Rio de Janeiro)

15–16. National Assoc. of Boards of Pharmacy, Washington, D.C. (P. H. Costello, 77 W. Washington St., Chicago, Ill.)

15–17. Heat Transfer Conf., ASME and

On May 18, 1960, in a ceremony at the National Academy of Sciences, the first of the newly established **ATOMS FOR PEACE** awards were presented. Two of the four pioneers in nuclear reactors so honored were **EUGENE P. WIGNER** and **ALVIN M. WEINBERG,** co-authors of **THE PHYSICAL THEORY OF NEUTRON CHAIN REACTORS.** Published by The University of Chicago Press in 1958, this volume was immediately hailed as "a magnificent accomplishment" (*Nucleonics*). If you do not already own this invaluable book, your bookseller will be glad to obtain a copy for you, or you may order on the coupon below.

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4. Neutron Cross-Sections
5. The Fission Process
6. Shell Structure of Nuclei and Giant Resonances
7. Neutron Chain Reactions
8. General Remarks about Diffusion Theory: Diffusion of Monoenergetic Neutrons
9. Transport Theory and Diffusion of Monoenergetic Neutrons
10. Energy Spectrum during Moderation
11. Diffusion and Thermalization of Fast Neutrons
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14. Applications to Specific Reactor Systems; Temperature Coefficients
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17. Reactor Kinetics
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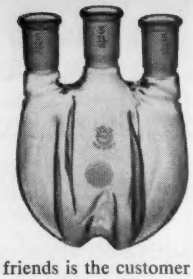

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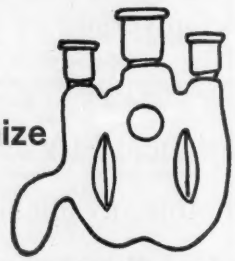

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AICE, Buffalo, N.Y. (A. B. Conlin, Jr., ASME, 29 W. 39 St., New York 18)

15–17. Organic Scintillation Detectors, intern. conf., Albuquerque, N.M. (G. H. Daub, Chemistry Dept., Univ. of New Mexico, Albuquerque)

15–18. American Veterinary Medicine Assoc., Denver, Colo. (H. E. Kingman, Jr., 600 S. Michigan Ave., Chicago 5)

15–18. Radiation Biology, 3rd Australian conf., Sydney, Australia. (P. Ilbery, Dept. of Preventive Medicine, Univ. of Sydney, New South Wales, Australia)

15–20. International Astronautical Federation, 11th cong., Stockholm, Sweden. (Secretariat, Intern. Astronautical Federation, 12, Bessborough Gardens, London, S.W.1, England)

15–23. Soil Science, 7th intern. cong., Madison, Wis. (R. Bradfield, Dept. of Agronomy, Cornell Univ., Ithaca, N.Y.)

15–24. Crystallography, intern. cong., Cambridge, England. (W. H. Taylor, Cavendish Laboratory, Cambridge, England)

15–25. Chemistry of Natural Products, IUPAC symp., Melbourne, Canberra, and Sydney, Australia. (Convener, Symposium Organizing Committee, Box 4331, G.P.O., Melbourne)

15–25. International Geological Cong., 21st session, Copenhagen, Denmark. (IGC, Mineralogical-Geological Museum, Univ. of Copenhagen, Øster Boldgade 7, Copenhagen K)

15–25. International Paleontological Union, Copenhagen, Denmark. (J. Roger, Service d'Information Geologique, B.R.G.-G.M., 74, rue de la Fédération, Paris 15°)

15–25. Sedimentology Cong., 6th intern., Copenhagen, Denmark. [General Secretary, IAS, c/o Institut Français du Petrole, 4, place Bir Hacheim, Rueil-Malmaison (Seine-et-Oise), France]

16–18. Biological Effects of Microwave Radiation, 4th annual conf., New York, N.Y. (M. Eisenbud, New York Univ. Post Graduate Medical School, 550 First Ave., New York 16)

16–19. Society of Automotive Engineers, San Francisco, Calif. (R. W. Crory, SAE, Meetings Operation Dept., 485 Lexington Ave., New York 17)

17–19. Hydraulics Conf., Seattle, Wash. (W. H. Wisely, American Soc. of Civil Engineers, 33 W. 39 St., New York 18)

17–19. University Nuclear Reactors, Gatlinburg, Tenn. (University Relations Div., Oak Ridge Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.)

17–21. Ionization Phenomena in Gases, 4th intern. conf., Uppsala and Stockholm, Sweden. (A. Nilsson, Fysikum, Uppsala)

18–19. Submarine and Space Medicine, 2nd intern. symp., Stockholm, Sweden. (H. Bjurstedt, Laboratory of Aviation Medicine, Karolinska Institutet, Stockholm 60)

20. American Inst. of Ultrasonics in Medicine, Washington, D.C. (D. M. Stillwell, Dept. of Physical Medicine and Rehabilitation, Univ. of Colorado Medical Center, Denver 20)

21–24. Latin-American Cong. of Angiology, Rio de Janeiro, Brazil. (R. C. Mayall, Caixa Postal 1822, Rio de Janeiro)

(See issue of 17 June for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ RECORDER of potentiometer type measures 11 by 14 by 4¾ in. and is designed for use on a laboratory bench or for wall mounting. It monitors d-c signals in the 10 to 100 mv range. Repeatability is said to be better than ±0.35 percent and full-scale response time 1.0 sec. Pen travel is 5 in. The pen drive used permits use of over-size strip chart or of circular charts. (Beckman Scientific and Process Instruments Division, Dept. Sci594, 2500 Fullerton Rd., Fullerton, Calif.)

■ FREQUENCY METER is a dual-range instrument reading either 396 to 440 cy/sec or 360 to 440 cy/sec from the same meter. Accuracy is said to be ±0.1 percent over the temperature range −55 to +65°C. The instrument uses semiconductor devices with no magnetic components. (Vidar Corporation, Dept. Sci604, 2107 El Camino Real, Palo Alto, Calif.)

■ TIMING BOARD for actuating three stop watches simultaneously permits timing of sequential steps with zero time loss between steps and without necessity for reading timer hands while they are in motion. Timing is started with the first watch at zero, the second stopped and ready to fly back, and the third in motion. Thus each step is timed in with the same motion with which the preceding step is timed out. (Heuer Timer Corp., Dept. Sci618, 441, Lexington Ave., New York 17, N.Y.)

■ PYCNOMETER permits determination of density of granular, irregular, or porous solids without use of liquids. The device operates by compressing air in two identical cylinders, one holding the sample to be measured contained in a cup. A third cylinder and piston communicating with the sample cylinder permits compensation for the effect of the volume occupied by the sample. This third cylinder is adjusted until the pressures in the two main cylinders, after compression, are equal. The motion of the third-cylinder piston required to effect the compensation is directly proportional to the volume of the sample. A choice of 50 cm^3 or 100 cm^3 sample containers is available. Accuracy of the instrument with a 50 cm^3 cup is said to be ±0.1 cm^3. (Houston Instrument Corporation, Dept. Sci620, P.O. Box 22234, Houston 27, Tex.)

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■ PHASE-ANGLE METER consists of an 8-watt power oscillator and a variable phase shifter having a range of 90 deg ±10 deg. Resolution and incremental accuracy are said to be ±0.05 deg and absolute accuracy better than ±0.25 deg throughout the range and better than ±0.1 deg at the 90-deg point. Standard frequency is 400 cy/sec, but other frequencies can be supplied. (Industrial Test Equipment Co., Dept. Sci622, 55 E. 11 St., New York, N.Y.)

■ CORROSION MEASUREMENT PROBE is a vacuum-deposited film of metal, 2 to 50 μin. thick, the metal varying with the application. As the metal exposed to the medium under test corrodes away, the resistance of the deposited metal increases. The change is observed by observing the ratio of resistance of exposed to protected sections of the film. As little as 5 mμin. of metal loss is said to be detectable. (Thompson Ramo Wooldridge, Inc., Dept. Sci613, Canoga Park, Calif.)

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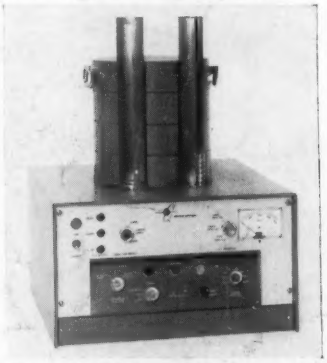

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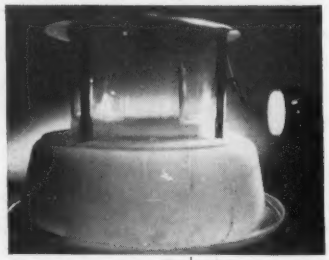

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